Do Strict Drink Drive Limits Save Lives?

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Abstract

Reducing drink drive limits is generally regarded an effective strategy to save lives. Using several new administrative data sources, we evaluate the effect of a stricter limit introduced in Scotland in 2014. This reduction had no effect on drink driving and road collisions, including fatal crashes. The unavailability of cheaper alternative means of transportation and weak law enforcement are the main channels behind the lack of an impact. We find no externality on many domains, from alcohol consumption to crime. Estimates from a supply-of-offenses function suggests that the reform did not have much ex-ante scope for sizeable effects.

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1. Introduction

Motivation — On the 5th of December 2014 Scotland reduced the legal drink drive limit (DDL) from 80 to 50 milligrams per 100 millilitres of blood.[1] The limit in the rest of the United Kingdom (England, Wales, and Northern Ireland) did not change and continues to be at the same level at the time of our analysis. While the Scottish limit decreased, the punishment for being caught and found guilty of driving above it remained unaltered both in Scotland and in the other three UK constituent countries. The first objective of this paper is to estimate the impact of the DDL reduction on road traffic collisions. After assembling many different administrative data sources, some never used before, and using a variety of nonexperimental research designs, we conclude that the reform had no impact on accidents. The second objective of the paper is to understand why this was the case. The unavailability of cheaper alternative means of transportation and weak law enforcement seem to have been the main channels behind the lack of an impact.

Motor vehicle accidents are a major public health problem worldwide. In 2016, there were 1.35 million road traffic deaths globally, with a heavier burden of these deaths falling on men and the young (World Health Organization, 2018). Even though there have been large reductions in automobile crashes over recent decades in the UK, they continue to be a considerable burden on health. For instance, in 2018, over 160,000 casualties from road traffic collisions were reported, consistently representing the second or third leading cause of death among Britons aged 5–34 in the last 30 years. Driving under the influence of alcohol is the main risk factor for road crashes everywhere, and a dose-response relation is systematically observed between blood alcohol concentration and road traffic accidents (Levitt and Porter, 2001). Medical research from field and laboratory studies suggests that lowering the BAC level from 0.08 to 0.05 would significantly curtail risk taking among drivers and stop people from being killed as a result (Fell and Voas, 2006; Breitmeier et al., 2007; Phillips and Brewer, 2011; van Dyke and Fillmore, 2017; Fell and Scherer, 2017).

Limiting BAC for drivers, therefore, has become a key policy tool used by governments across the world in their attempt to save lives on the road. Having a limit of 0.05 BAC is one of the criteria for best practice set out by the World Health Organization (WHO). To date, 45 countries representing 2.3 billion people have drink drive laws that align with WHO best practice. In the US, since 2017, all states have a standard limit of 0.08 BAC (except Utah, which lowered it to 0.05 at the end of 2018), with some states adopting policies of lower limits for young or novice drivers. An overall abatement to 0.05 BAC has been discussed but not yet agreed (e.g., National Transportation Safety Board, 2013). In 2001, the European Commission recommended that all member states should adopt a legal maximum limit of 0.05 BAC or lower, for drivers and riders of all motorized vehicles (Official Journal of the European Communities, 2001).[2] In the UK, the government-commissioned *North*

[1] As it is standard in the literature, we shall refer to such measures in terms of blood alcohol concentration (BAC) expressed in grams of alcohol per deciliter of blood, that is, 0.08 BAC and 0.05 BAC, respectively. Alternatively, these same two figures also correspond, respectively, to 35 micrograms (μg) of alcohol per 100 millilitres (ml) of breath and 22 μg/100 ml, or to 107 milligrams (mg)/100 ml and 67 mg/100 ml of alcohol in urine.

[2] This was followed up ten years later by the European Parliament asking the Commission to prepare EU-wide

Review on the legal framework of drink and drug driving concluded with the recommendation to cut the legal maximum BAC from 0.08 to 0.05 (North, 2010). With the 2014 DDL reform, Scotland adopted this recommendation, with the explicit intent to make roads safer and save lives. As of July 2020 among all European countries, only England, Wales, and Northern Ireland have a standard drink drive limit greater than 0.05 BAC.[3]

What does a reduction from 0.08 to 0.05 BAC mean in practice for an average consumer planning to drive? Although this is difficult to pin down with precision, most of the information available to the public suggests that a healthy adult man of mean weight can consume two pints of beer (or two glasses of wine) over a meal and be below the 0.08 BAC limit.[4] The stricter 0.05 BAC threshold implies a reduction to one pint of beer or one glass of wine. For a healthy adult woman of mean weight, all figures should be roughly halved.

One critical implication of this reduction is for the brewing and distilling sectors, which play a vital role in the Scottish economy, in terms of employment, links to upstream and downstream sectors, and turnover, contributing approximately 3.5% to total Scottish GDP on average since 2000 (O'Connor, 2018).[5]

Scotland's case is important also because of the country's historically high mortality rates across all ages compared to the rest of Europe and most other industrialized economies. For women and men aged 15–74, Scotland has recorded the highest all-cause age-standardized mortality rates since the early 1980s, including liver disease and cirrhosis, which are specific causes of death typically associated with heavy drinking (Whyte and Ajetunmobi, 2012). Consequently, life expectancy in Scotland, whether at birth or at age 65, has been among the lowest among OECD countries, with gaps of at least 4–5 years with respect to several economies, including France, Germany, Italy, Canada, Israel, Japan, South Korea, and Australia (OECD, 2019; National Records of Scotland, 2019). Some of the arguments leading up to the 2014 reform were motivated by this hard pre-existing reality (Granville and Mulholland, 2013; Scottish Health Action on Alcohol Problems, 2014).

Our contributions — The explicit goal of the lower drink drive limit stated by Scottish policy makers was to save lives, improving safety on the roads, reducing road traffic accidents and deaths, and lowering convictions.[6] Economic theory suggests that, ceteris paribus, if the actual or perceived

proposals for a harmonised blood alcohol limit and a BAC limit of zero for novice and professional drivers (European Transport Safety Council, 2016).

[3]See <https://etsc.eu/blood-alcohol-content-bac-drink-driving-limits-across-europe/>.

[4]Among others, see North (2010) and <https://www.alcohol.org/bac-calculator/>.

[5]On the 1st of May 2018, Scotland passed another ethanol related reform, which introduced a minimum unit price (MUP) of alcohol at 50 pence per unit. The MUP was initially supported by the Scottish Parliament through the Alcohol Act 2012. But it was legally challenged by the Scotch Whisky Association and referred to the European Union Court of Justice. The response in December 2015 (after the 2014 DDL reform) required Scottish judges to consider whether alternative tax policies were ineffective in protecting public health. After almost two years, on 15 November 2017, the Supreme Court of the United Kingdom rejected the Scotch Whisky Association's case, arguing that minimum pricing was a proportionate means of achieving a legitimate aim. This reform falls outside our period of interest, and therefore is not part of our analysis.

[6]For official statements from the Justice Secretary, the Assistant Chief Constable of Police Scotland, and the Director of Road Safety Scotland, see <https://www.wired-gov.net/wg/news.nsf/articles/Lower+drink+drive+limit+in+Scotland+04122014102005?open>. See also the discussion in the next section.

probability of conviction goes up (because the DDL has been lowered) or the probability of punishment increases, we should expect to see a reduction in drink drive offenses and, because of the BAC-collision relationship illustrated by the empirical literature, a reduction in motor vehicle accident rates (Becker, 1968; Ehrlich, 1973; Becker and Murphy, 1988; Sah, 1991; Levitt and Porter, 2001). In the DDL context, alternative models based on behavioral insights may lead to similar predictions on traffic collisions, even though they could identify different mechanisms.[7] The BAC limits, in fact, are set at levels low enough that might not require us to invoke cognitive impairment, limited self-control, weak wills, or the operation of visceral influences on drink driving. Nevertheless, testing one theory against another goes beyond the scope of this paper. Rather, we aim to provide a comprehensive evaluation of the reform on the one hand and a unifying, rigorous understanding of the channels behind its effects, or lack of, on the other. For this latter aspect of the analysis, we use intuitions from both standard theory and behavioral models.

To evaluate the impact of the reform, we rely on highly detailed geographical data and new administrative information defined at the level of local authorities, of which there are 347 in England and Wales and 31 in Scotland.[8] In the attempt to address the issue of noncommon trends between Scotland and the rest of Britain as persuasively as possible, limit the role played by unobserved local differences, and reduce the scope for omitted variables bias, we compare the results from three approaches, namely, models based on difference-in-differences, spatial regression discontinuity design, and synthetic control methods. We supplement both the difference-in-difference analysis using a linear panel event-study design following Freyaldenhoven, Hansen, and Shapiro (2019) and the synthetic control approach combining matching and synthetic control estimators through model averaging as proposed by Kellogg et al. (2019). All estimates reveal that the lower Scottish limit had no impact on any type of road accident, from fatal crashes to collisions involving just slight injuries. For the first time for Britain, we have access to data with alcohol involvement and confirm the null result also for drink drive accidents.

This evidence holds true for various subgroups of the population (e.g., young men), and whether we consider nights, weekends, or multiple vehicle crashes. It is also robust to different definitions of the outcome variable, different functional forms used in estimation, randomization inference, and alternative definitions of drink drive collisions.

Despite the zero-effect result, examining mechanisms is all the more important, since we may not only understand behavior better but also inform policy more precisely. To guide the interpretation as to why the DDL reduction to 0.05 BAC was ineffective, we go back to the framework proposed by Becker (1968) and Ehrlich (1973 and 1996), in which potential lawbreakers decide whether to engage in drink driving by carrying out a cost-benefit calculation under uncertainty. Individuals

[7]These include cognitive limits on decision making (George, Rogers, and Duka, 2005), lack of self-control (Bettman, Luce, and Payne, 1998), visceral factors affecting judgement (Loewenstein, 1996), social pressure to conform (DellaVigna, 2009), and present-focused preferences (Ericson and Laibson, 2019). For an overview of models in the drink drive setting, see Sloan, Eldred, and Xu (2014).

[8]Since most the data we have do not cover Northern Ireland, this country is not part of our analysis. We therefore focus on Britain only.

assess whether the expected benefit from drink driving, accounting for the probability of being caught, outweighs the expected cost. In this setup, drink driving is affected by both direct and indirect economic incentives (Draca and Machin, 2015). Among the direct incentives, we consider alternatives to driving under the influence, and explore changes in the availability of alternative means of transportation (e.g., taxis and buses) and their fares. A key indirect mechanism is through deterrence and incapacitation effects. Although the reform did not change the punishment for drink driving, we examine whether the lower limit was accompanied by changes in enforcement, specifically police numbers and breath testing at the roadside, including tests unrelated to road collisions, as well as drink drive arrests and convictions unrelated to traffic accidents. We find that alternative means of transportation were neither more available nor cheaper, and police enforcement was weak. Both channels therefore contribute to the lack of an impact of the reform.

Building on the insights from both the conventional economic model of crime and behavioral models, we then explore the possibility of unintended consequences of the stricter limit. We find the reform led to a greater anti-drink driving sentiment among the public, although this was not enough to induce people to scale back their alcohol consumption or reduce own vehicle usage. There was no increase, but also no abatement, in other types of offenses and criminal activities, including speed limit violations, illegal drug-related crimes, serious assaults, and sexual offenses. We also find no appreciable price and quantity responses from the alcohol and automobile industries, which might have been negatively affected by the reform. Finally, estimating a reduced form version of the supply of offenses, which links BAC levels to the number of road traffic violations and accidents, reveals only a small alcohol intake-collision elasticity over the 0.05–0.08 BAC range. This implies that the reform did not have much scope for a sizeable impact. The pre-existing DDL was already sufficiently low that further reductions in motor vehicle accidents could not be readily achieved.

Related Literature — There is a wide economic literature on the impact of various alcohol policies on motor vehicle collisions, with a particular emphasis on traffic fatalities.[9] One strand focuses on the impact of alcohol taxes with findings suggesting a negative association (e.g., Cook, 1981; Chaloupka et al., 1993; Ruhm, 1996). Another takes advantage of the changing legal status of drinking by age. For the United States, Carpenter and Dobkin (2009, 2011, and 2015) find increases in mortality around the age when drinking becomes legal. This increase seems to be driven, in part, by an increase in fatal crashes which go up by 15% at the age of 21.[10] Using the census of judicial records on criminal charges filed in Oregon courts, Hansen and Waddell (2018) find that crime increases at age 21, especially in the case of assaults lacking in premeditation, alcohol-related offenses, and drink driving.[11] This evidence is suggestive more of the impact of heavy alcohol use than of the

[9] An earlier review of the empirical evidence on drink driving is given by Benson, Rasmussen, and Mast (1999). Sloan (2020) provides a comprehensive update.

[10] A similar effect of around 17% is found for Canada, this being driven mainly by men (Carpenter, Dobkin, and Warman, 2016).

[11] These results are broadly confirmed by Fletcher (2019), who finds large detrimental effects of alcohol access on drink driving, violence, and other risky behaviors, especially among men in the Add Health data. Examining the

impact on margins that are closer to legal drinking levels, which DDLs attempt to influence.

Other studies exploit variation in the availability of alcohol through restrictions (or relaxations) on the times of the day or days of the week when alcohol can be sold. Liberalization of Sunday alcohol laws (also known as "blue laws") have been shown to have had mixed effects on traffic fatalities (McMillan and Lapham, 2006; Lovenheim and Steefel, 2011).[12] Stehr (2010) exploits changes of blue laws in 14 states using data from 48 states from 1995 to 2008 and concludes that the repeal led to an increase in highway crash fatalities only in New Mexico. Using variation in the legalization of Sunday packaged alcohol sales across Virginia, Heaton (2012) finds no impact on arrests for driving under the influence. In 2005, England and Wales liberalized bar opening hours. Green, Heywood, and Navarro (2014) find that this change in on-trade availability led to a *decrease* in road accidents, with the fall concentrated among the young and at times when the policy was expected to have most bite (i.e., at the weekends and during more popular drinking hours). Restrictions of liquor availability in Brazil, in contrast, led to a decline in deaths by car accidents (Biderman et al., 2010).[13]

A smaller strand of research focuses on the effect of BAC limits on road accidents, mainly in the United States. Carpenter (2004) analyzes the impact of zero-tolerance laws for the young, which set stricter BAC limits for individuals under age 21. The results indicate these laws led to a reduction in heavy drinking (for men), but not drink driving. Among the closest papers to ours, Dee (2001) examines the effect of lowering DDLs for all drivers. Using variation across 48 continental states from 1982 to 1998, he finds that lowering the limit to 0.08 BAC reduced fatal accident rates by 7.2%, implying roughly 1,200 lives saved annually. Eisenberg (2003) confirms this result, estimating that a decline in the legal limit from 0.10 to 0.08 BAC reduced fatal crash rates by 3.1% percent, although it might take a few years for the effect to be observed. Using data on 15 European countries observed between 1991 and 2003 and taking advantage of the reduction in the BAC level down to 0.05, Albalate (2008) finds evidence of a decrease in fatal road accident rates for young drivers in urban areas, but no overall effect.

Two other papers examine the 2014 BAC reduction in Scotland on road accidents estimating difference-in-difference models. Haghpanahan et al. (2019) use weekly motor vehicle accident rates calculated at the *country* level (i.e. one rate for Scotland and one for England and Wales), while Cooper, Gehrsitz, and McIntyre (forthcoming) use less aggregated data, comparing the whole of Scotland to ten other large regions in England and Wales. Neither study finds evidence that the

reduction in legal drinking age from 20 to 18 years in New Zealand, Boes and Stillman (2013) document an increase in alcohol related hospitalizations, but no increase in alcohol related road accidents.

[12] Anderson, Crost, and Rees (2018) show that greater alcohol availability (measured by the increase in the number of establishments licensed to sell alcohol by the drink in the state of Kansas) is associated with a 3 to 5% increase in violent crime, such as murder, rape, and robbery. Road accidents and drink driving, however, are not included in their research. Seim and Waldfogel (2013) provide additional analysis of how governments attempt to curb problematic alcohol consumption through restricting availability.

[13] Jackson and Owens (2011) investigate the impact of public transport on fatal road accidents, drink-drive arrests and alcohol related arrests. They exploit the late night metro service in Washington DC and find little overall effect. They find, however, significant heterogeneity, with an increase in alcohol related arrests and a decrease in drink-drive arrests in areas where bars are within walking distance to transit stations.

change in the law led to lower road accident rates. One concern with both studies, however, is that it is not obvious that Scotland and the rest of Britain shared a common trend prior to the reform. As we document in Section 3, accident rates were on a downward trend in both areas, but Scotland's decline was faster even before the enactment of the stricter limit. Moreover, the high levels of aggregation in both studies can mask important differentials within each constituent country.

Our analysis also speaks to the growing body of research that argues that changing defaults can improve wellbeing (Johnson and Goldstein, 2003; Gigerenzer and Brighton, 2009), including the debate on organ donations (Becker and Elias, 2007; Kessler and Roth 2014), green electricity (Pichert and Katsikopoulos, 2008), and retirement saving (Beshears et al., 2009). In our case, tightening the DDL default was ineffective in abating road fatalities.

The remainder of the paper is organized as follows. Section 2 outlines the institutional background surrounding the introduction of the 2014 Scottish reform and discusses some of the key factors that affect BAC levels through the way ethanol is absorbed and metabolized. Section 3 describes the data and methods used for the policy evaluation. Section 4 presents the benchmark estimates of the evaluation, checks for heterogenous effects, and shows the results from a broad set of sensitivity exercises and from drink drive crimes unrelated to road accidents. Section 5 explores the main mechanisms suggested by a market model of crime, from alternatives to drink driving to police enforcement of the new limit. Section 6 investigates whether the reform had unintended consequences on multiple domains, such as public attitudes toward drink driving, alcohol consumption, healthy eating and smoking habits, and own vehicle usage, or if it generated undesired spillovers to other crimes and offenses, and the alcohol and automobile industries. Section 7 discusses the results obtained from the estimation of a supply of offenses, which links motor vehicle violations and accidents to BAC levels. Section 8 summarizes the main findings and provides a few pointers for public policy discussion. Supplementary material on the data and additional results discussed throughout the paper are in the Online Appendix.

2. Background

The legal limit of 80mg of alcohol per 100ml of blood was set out in the 1967 British Road Safety Act and had not changed in over 45 years. On the 18th of November 2014 the Scottish Parliament voted unanimously to reduce the legal drink driving limit in Scotland, lower than elsewhere in the UK. The new law, which brought Scotland in line with other European countries, came into force on the 5th of December 2014. The reduction from 0.08 to 0.05 BAC was not accompanied by any change in the punishment (measured in terms of fines, penalty points, driving disqualifications, and jail sentences) associated with breaking the law.[14]

[14] For penalties, see <https://www.gov.uk/drink-driving-penalties>. The divergence between Scotland and the rest of the UK was made possible by the 2012 Scotland Act, which gave the Scottish Parliament and Ministers further powers, including the ability to change the drink drive limit. See <https://https://tinyurl.com/devolvedpowers>.

Aside from the amount of alcohol consumed, the determination of one's BAC is a function of a number of pharmacokinetic factors, including gender, weight, age, absorption, and speed of consumption, which cannot be easily accounted for by drinkers (Baraona et al., 2001; Koob, Arends, and Le Moal, 2015). For instance, faster alcohol consumption is typically associated with quicker rises in BAC. All else equal, BAC among women is higher than among men, since women on average have less water in the body and BAC is proportional to the total body water content. For a given level of alcohol consumed, high-weight people show smaller BAC than their low-weight counterparts, since they have more water content in the body. Similarly, fatter individuals may experience higher BAC, because fatty tissues do not absorb alcohol well. Older people take longer to metabolise alcohol than younger people. Food and medicines can also lead to differences in alcohol absorption rates. Finally, the rates of metabolizing alcohol differ from person to person. On average, 10ml of alcohol is metabolised per hour. However, heavy drinkers can metabolize alcohol faster than light drinkers and thus, for a given amount of alcohol over the same amount of time, they will have lower BACs.

The reduction to 0.05 BAC obviously cuts the amount of alcohol that can be consumed. But it is unclear by how much. Given the factors discussed above, it is hard to determine precisely the level of BAC for a given amount of alcohol consumed. Appendix Figure A.1 shows the relationship between number of drinks consumed over a one-hour period for an average man aged 40 weighing 84kg and an average woman aged 40 weighing 70kg. For the man, drinking two pints of 5% alcohol-by-volume beer over one hour would be under the legal limit in England and Wales but over the lower default in Scotland. For the woman, one pint would put her very close to the new Scottish limit, while two pints would take her clearly over both standards. These figures are in line with the *North Review*, which states that the 0.05 BAC limit "would still allow the responsible driver who wishes to enjoy a drink to accompany their pub meal or have a glass of wine or a pint of beer to do so without being in danger of breaking the law" (North, 2010, p. 7 and p. 96). It is nonetheless very difficult for people to judge the level of alcohol in their body accurately.

The new law in Scotland was accompanied with a massive TV advertising campaign in the weeks around the introduction of the reform, aimed at raising awareness of the new law for all Scottish citizens and particularly those living near the border. ITV Borders, which broadcasts to the south of Scotland and the north of England, ran adverts starting from 17 November 2014 up to 2 January 2015.[15] This was backed by an intense and coordinated public information campaign on all other TV channels, radio stations, and social media, by electronic road signs across Scotland and on key border roads between England and Scotland, and by posters and digital screens in all Scottish public venues, such as train stations,[16] main airports, medical centers, supermarkets, tourist information

[15]The campaign message was "The best advice is none, when it comes to drinking and driving." Some of the adverts are available at: <https://www.youtube.com/watch?v=aPfpvfAWX68&feature=youtu.be> and https://www.youtube.com/ watch?v=Qd3J8rekAfw&feature=youtu.be>. More sources can be found in the Online Appendix.

[16]Just the two central stations in Edinburgh and Glasgow see an average footfall of about one million commuters and visitors per week.

agencies, car hire companies, petrol pumps and garages, and all alcohol retailers.

With a contribution in excess of 3.5% to the Scottish GDP, the alcohol industry plays a key role in the country's economy. Soon after the introduction of the lower limit, many commentators claimed the reform depressed alcohol consumption in pubs and restaurants and even had a direct effect on the slower economic activity registered in Scotland in the first quarter of 2015 (e.g., Green, 2015; Wright, 2015). In this environment and from a broader perspective, it is worth stressing that the new DDL was introduced in the aftermath of the global financial crisis with the UK government pursuing economic austerity and severely cutting public service spending in real terms (Crawford and Zaranko, 2019).

3. Data and Methods Used for the Policy Evaluation

Data Sources — For the evaluation of the 2014 DDL reform, we use several data sources.[17] The first, which serves as the main input for our dependent variables, is the Road Accidents Data (RAD), the British official administrative source for all motor vehicle collisions reported to the police and recorded using the STATS19 accident reporting form. The RAD are collected by police officers on behalf of the Department for Transport (DfT) whenever an accident involves at least one personal injury, however minor this might be. We use all monthly records from January 2009 to December 2016 on over 1.2 million accidents. Each record contains details about the accident and the individuals involved, including their age and sex, the exact time and location of the accident, and its severity, and this in turn is distinguished into fatal, serious, and slight.

The data also contain information on alcohol involvement, which has never been used before. This information was collected from surviving drivers or riders, who were breath tested at the roadside. The level of alcohol in the breath is not given, but we know whether the test was negative, positive, or whether the driver(s) refused to take the test. A drink drive accident is defined as an incident reported on a public road in which someone was killed or injured (even slightly) and at least one of the drivers involved met one of the following criteria: (i) failed a roadside breath test by registering above 35μg of alcohol per 100ml of breath in England and Wales, or 22μg/100ml in Scotland from December 2014 onwards (and above 35μg/100ml before December 2014 in the whole of Britain); or (ii) refused to give a breath test specimen when requested by the police, except when incapable of doing so for medical reasons.[18]

The cross-sectional unit of observation is the local authority district (or local council). In Britain there are 378 local authorities in total, 347 in England and Wales and 31 in Scotland. Our outcomes are accident rates (either total, by level of severity, or drink drive), defined as the number of accidents

[17] Others will be described in the following sections. For additional details on all data sources used, see the Online Appendix.

[18] In supplementary checks, we complement this analysis using information on drink drive accidents adjusted by the DfT for underreporting due to death and additional data from coroners in England and Wales and public prosecutors in Scotland. These data, however, have a greater level of aggregation, in terms of accident time and location. See Section 4.

for each category in a given local authority and a given month per 1,000 vehicles registered in the same local authority.[19]

Figure 1 shows the monthly accident rates observed over the sample period by country (i.e., Scotland versus England and Wales) averaged over all local councils for all motor vehicle collisions. Figure 2 displays the accidents rates by type. We stress two points. First, Scotland experienced lower crash rates than the rest of Britain. This is especially clear for all accidents and slight injury accidents, but it is not obvious in the case of fatal crashes. Second, the introduction of the reform does not seem to be followed by a noticeable slowdown in collision rates in Scotland, for all accidents together and each of the four specific types. These two observations suggest that the DDL reform took place in an environment with already lower accident rates where the stricter limit might have had no impact. Accounting for seasonality effects yields the same results (see Appendix Figure A.2).

Road traffic collisions are likely to be correlated with a variety of factors other than BAC, which we control for in the analysis. These are: (a) weather conditions, proxied with the monthly regional average temperature range, i.e., the difference between the maximum and minimum temperatures in degree Celsius, recorded in each UK climate region (obtained from the Metereological Office);[20] (b) road congestion, which is proxied by council-level population density and road length in kilometers (obtained respectively from the Office for National Statistics (ONS) and DfT);[21] (c) socioeconomic status of the population in each local authority, which is measured in terms of four separate domains, that is, the proportion of residents aged 16 or more with no educational qualification (from the 2011 Census), the proportion of individuals with bad or very bad health (2011 Census), the median total hours worked and the fraction of residents aged 16–64 claiming Job Seeker's Allowance (both obtained from NOMIS, the ONS labor market statistics); and (d) the availability of alcohol, proxied by the total number of licensed alcohol premises in each local council (obtained from the UK Department for Digital, Culture, Media and Sport, the Home Office, and the Scottish Government).

Summary statistics of the pre-reform outcomes and explanatory variables are reported in Table 1, where we distinguish between treatment and control groups for the three main statistical methods used in the analysis (see below). Confirming the evidence shown in Figures 1 and 2, monthly accident rates were on average lower in Scottish districts (0.27 versus 0.40 per 1,000 registered vehicles), mainly due to smaller value for slight accident rates (0.22 versus 0.34). However, differences in outcomes are virtually eliminated when looking at the subsamples used with the synthetic control design. Significant differences between Scotland and their English and Welsh counterparts emerge

[19]The information on the time series of the number of vehicles by local council comes from the DfT. As discussed in the next section, we also use other definitions of rates based on different populations at risk, such as the entire population and road availability.

[20]We performed the analysis also using separately minimum and maximum temperatures as well as the monthly amount of rainfall (in millimetres), the number of days with rainfall greater than 1 mm, the number of days in which air frost was recorded, and the total monthly number of hours of sunshine. Since most of the results were virtually identical to those presented below, we opted for a more parsimonious specification.

[21]Density is defined as the mid-year population estimate for individuals aged 17 (the age at which individuals can start driving in the UK) or more divided by the local authority area measured in hectares.

also for nearly all the explanatory variables, irrespective of the distance from the border. For instance, Scottish local councils had a lower population density and a greater fraction of residents with no educational qualifications, working fewer hours, in bad health, and claiming unemployment benefits. Again, most of such differences shrink substantially when we consider the subsamples selected by the synthetic control approach.

Methods — Our goal is to evaluate whether the Scottish 0.05 BAC law was effective in saving lives on the road. To this end we employ three separate (but related) statistical designs. The first is a standard difference-in-difference (DD) model. Letting y_{cm} be the road accident rate in local authority c in month m, S_c an indicator variable equal to 1 if local council c is in Scotland and 0 otherwise, and $\mathbb{I}(z)$ a function indicating that the event z occurs, the DD model is given by

$$y_{cm} = \alpha_0 + \alpha_1 S_c + \alpha_2 \mathbb{I}(m \geq \tau) + \beta S_c \times \mathbb{I}(m \geq \tau) + \psi(t) + \theta_c + \mathbf{X}'_{cm}\gamma + \varepsilon_{cm}, \tag{1}$$

where τ coincides with the month-year when the reform was introduced (December 2014), $\psi(t)$ denotes time fixed effects, θ_c refers to local council fixed effects, and $\mathbf{X}_{cm}$ is a vector of possibly time varying characteristics at the local authority level that can affect accidents, including the monthly regional average temperature range, population density, road length, the socioeconomic status of the population, and the number of alcohol premises. To account for pre-reform trend differences and seasonality in accident rates, we allow the $\psi(t)$ function to include both group-specific linear month-year trends and group-specific month dummies.[22] Finally, we consider five different outcomes, that is, all types of collisions, crashes by severity (i.e., fatal, serious, and slight accidents), and drink drive accidents.

As discussed in the Introduction and illustrated in Figures 1 and 2, the trends in Scottish road accident rates were different from the trends in the rest of Britain, even before the enactment of the new DDL in December 2014. As an alternative way to account for pre-reform trends, we therefore supplement the DD approach using the linear panel event-study design suggested by Freyaldenhoven, Hansen, and Shapiro (2019). This model allows for unobserved confounders to be correlated with both the outcome variables and the 2014 reform.

In the second approach, we take into account the fact that the differences in accident rates and their determinants between Scotland on the one hand and England and Wales on the other may be driven by unobserved local (geographic) differences. In order to address this potential concern, we employ a spatial regression discontinuity (henceforth, spatial RD) design framework combined with the previous DD approach. The idea is to give more weight to observations that are closer to the Scottish-English border versus those farther away. Besides a better alignment in weather conditions, which are controlled for in (1), such a comparison is likely to pick up unobserved or hard-to-measure cultural similarities between neighboring Scottish and English regions, such as food and drink norms, recreational habits, and attitudes toward the law. As in other spatial RD applications,

[22] Additionally, we have tried using quadratic trends and the results are quantitatively similar.

the running variable is the distance from the border, specifically the Euclidean distance between the centroid of each local authority and the border (e.g., Black 1999; Lalive 2008; Campa and Serafinelli 2019). Following Gelman and Imbens (2019), we estimate a local linear RD polynomial which controls linearly for distance from the border and weights local authorities by proximity to the border using a triangular kernel. In particular, for the same five outcomes mentioned above, we estimate

$$
\begin{aligned}
y_{cmb} \quad = \quad & \alpha_0 + \alpha_1 S_c + \alpha_2 \mathbb{I}(m \geq \tau) + \beta S_c \times \mathbb{I}(m \geq \tau) \\
& + \delta_1 \text{Distance}_c + \delta_2 \text{Distance}_c \times S_c + \psi(t) + \theta_c + \mathbf{X}'_{cmb}\gamma + \epsilon_{cmb},
\end{aligned}
\tag{2}
$$

where Distance_c is distance from the Scottish-English border, with English distances taking negative values. The weights we use are equal to $\max\{h - |\text{Distance}_c|\}$, where h denotes the bandwidth of local authorities around the border. We will present results for $h = \{200\text{km}, 100\text{km}, 50\text{km}\}$.

To further contain the scope for omitted variable bias and increase the similarity between treatment and control local councils, the third design follows the synthetic control method introduced by Abadie and Gardeazabal (2003) and Abadie, Diamond, and Hainmueller (2010). With this approach, we weight local authorities in the control group to construct a synthetic counterfactual that replicates the basic predictors of accidents for Scottish local authorities before the 2014 DDL reform (see also Abadie, Diamond, and Hainmueller [2015]).

Adjusting the previous notation to Rubin's (1974) potential outcome framework, we define y^1_{cm} as the accident rate in month m if the local council c is in Scotland and y^0_{cm} the corresponding outcome if the local authority is not in Scotland, so that the treatment effect of the reform is given by $\beta_{cm} = y^1_{cm} - y^0_{cm}$. The synthetic control estimator compares the outcome in the treated region (Scotland) averaged over all local councils, y^1_m, to a weighted average of the outcome over all local authorities in the control group, that is:

$$
\widehat{\beta}_m = y^1_m - \sum_{c \in C} \omega_c y^0_{cm}, \tag{3}
$$

where $\omega_c \geq 0$ is the weight attached to each local authority c in the control group C. Since treated and control units are observed in different states after the introduction of the reform at month τ (i.e., with and without the 0.05 BAC law, respectively), (3) becomes

$$
\widehat{\beta}_m = \beta_m + \Big(y^0_m - \sum_{c \in C} \omega_c y^0_{cm} \Big), \quad \text{for all } m \geq \tau. \tag{4}
$$

The accuracy of this approach therefore relies on minimizing the difference in parentheses in (4). A way to achieve this is to minimize the difference between treated and control local councils over the pre-reform period, when none of them was exposed to the reform. As long as the weights reflect features that do not change in the absence of the DDL reduction, the synthetic control approximates the (unobserved) counterfactual evolution of the potential outcome y_m^0 from τ onwards.[23] Specifically, let $\mathbf{X}_c^1$ and $\mathbf{X}_c^0$ be the vectors of collision determinants for the treated region (Scotland) and for each of the local authorities c in the control group, respectively. The optimal vector of weights will minimize the square distance $(\mathbf{X}_c^1 - \sum_{c \in C} \omega_c \mathbf{X}_c^0)' \mathbf{V} (\mathbf{X}_c^1 - \sum_{c \in C} \omega_c \mathbf{X}_c^0)$, where $\mathbf{V}$ is a diagonal matrix with non-negative entries measuring the relative importance of each predictor, $\omega_c \geq 0$, for all $c \in C$, and $\sum_c \omega_c = 1$. In turn, the optimal matrix $\mathbf{V}^*$ is chosen to minimize the mean squared error of outcomes over the pre-reform period, that is, $\frac{1}{m^0} \sum_{m<m^0} (y_m^1 - \sum_{c \in C} \omega_c^* y_{cm}^0)^2$, for $m^0 < \tau$. In the evaluation, the outcome of interest is accident rates and, as described above, the $\mathbf{X}$ vector includes key predictors of accidents, namely council-level weather conditions, road congestion, socioeconomic status of the population, and alcohol availability. To compute both the weights and the mean squared prediction error needed in the procedure we use the entire pre-intervention period from November 2009 to November 2014, 61 months in total.

An important advantage of the synthetic control method over the DD and spatial RD models is that it limits extrapolation bias, which can emerge when untreated local authority districts (in England and Wales) display different pre-reform characteristics and trends with respect to their treated (Scottish) counterparts. As observed by Abadie, Diamond, and Hainmueller (2010), however, the synthetic control estimator may suffer from interpolation bias as it uses a weighted average of the untreated local councils to create a synthetic untreated Scotland with pre-reform characteristics similar to those observed for Scotland. Other estimators instead, such as nearest-neighbor matching, have the opposite properties, that is, they curb interpolation bias but suffer from extrapolation bias, extrapolating too much when suitable untreated districts are unavailable. Kellogg et al. (2019) suggest to optimize the strength of the two estimators and combine matching and synthetic control (MASC) procedures through model averaging. In a sensitivity analysis, we follow this more recent approach and present evidence based on the MASC estimator.

4. Results on the DDL Reform Evaluation

A. Benchmark Estimates

Table 2 reports the DD estimates of the effect of the 2014 Scottish 0.05 BAC law using the STATS19 information contained in the RAD records. We show the results from five different specifications of (1), depending on whether we include controls, group-specific linear month-year trends, group-specific month fixed effects, and local authority fixed effects, and for five different definitions of the outcome variable.

The estimates in panel A, where we examine all cases included in the RAD records, reveal there is a 1.3 percentage point (nearly 5%) reduction in total accident rates as a result of the reform.

[23] As pointed out by Abadie, Diamond, and Hainmueller (2010), an analogous identifying assumption, namely that unobserved differences between treated and non-treated local authorities are time-invariant, is also imposed by the DD model described above. In fact, the synthetic control method generalizes the DD model by permitting the effect of unobserved confounders to vary over time according to a flexible factor representation of the potential outcomes of the treated local authorities.

But this is true only when we consider the raw DD specification (column (a)). Including controls in column (b) leads to an insignificant estimate, while including month-year trends, month fixed effects, and local authority fixed effects leads to small positive and statistically insignificant effects (columns (d) and (e)). We also find a 13% reduction in serious injury accident rates (panel C), but this impact disappears when we control for time trends, month and local authority fixed effects (specifications (c)–(e)). Irrespective of the specification, all the estimates for fatal and slight injury accidents are quantitatively modest and statistically insignificant (panels B and D, respectively). Thus, it is not the case that the tighter BAC limit, which might have had a greater influence on low levels of alcohol intake, had an impact on less severe crashes. Finally, and crucially, we find no effect on drink drive collisions across all five specifications (panel E). Overall, therefore, there is no convincing evidence that the 2014 reform has had any impact on accident rates in Scotland.

The spatial RD estimates from equation (2) are presented in Table 3 showing results for progressively smaller bandwidths, from 200 down to 50km.[24] The reform did not have any significant impact on accident rates as a whole, as well as on serious and slight accident rates. We find an effect on fatal accidents and those accidents with at least one driver who had a positive (or refused) breath test, but this emerges only when we consider further distances from the border (up to 200km). By and large, therefore, these results uphold those found earlier.

Figure 3 and Appendix Figure A.6 display maps of Britain, in which the light shaded areas represent English/Welsh local authority districts in synthetic Scotland along with their weights, for all motor vehicle collisions and each of the other four accident types, respectively. All unshaded districts in the potential control group are assigned zero weights. Figures 4 and 5 show district-specific accident rates (all and by type, respectively) for Scotland and synthetic Scotland. They suggest the reform had essentially no effect on all crash rates and by type. The estimates for synthetic Scotland closely track the trajectory of accident rates in Scotland for the whole pre-intervention period. But, after the enactment of the Scottish 0.05 BAC law, the two lines continue to overlap substantially regardless of the type of collision, revealing that the alcohol restriction led to no change in road traffic accidents.

As proposed by Abadie, Diamond, and Hainmueller (2010), appropriate inference can be established by performing a falsification test based on the distribution of the (placebo) effects estimated for all local authority districts in the control group. The null hypothesis that the effect of the DDL reform is equal to zero is rejected if the effect estimated for the Scottish districts is abnormal relative to the distribution of placebo estimates. If instead the distribution of placebo effects yields effects that are similar to those found for synthetic Scotland, then it is likely that the DDL reduction did not have any impact. We therefore replicate the synthetic control estimates for all possible sets of local authority districts in the control group, pretending that each placebo district experienced the

[24] In square brackets, Table 3 reports wild bootstrapped p-values of the treatment effects, which are estimated when the spatial dimension (number of local councils) is small (Webb, 2014). For completeness, however, we report p-values for all bandwidths. The maps in Appendix Figure A.3 displays how the control areas change as we vary the distance from the border.

treatment in December 2014. Clearly, it is possible that some of the placebo effects are implausibly large if councils are not well matched in the pre-intervention period. To control for this, we restrict the comparison set of local authority districts to only those that match well and remove all the comparisons with a pre-treatment mean squared prediction error (MSPE) that is more than two times the corresponding MSPE found with the synthetic control.[25]

The results are reported in Figure 6, which shows the distribution of estimates for the placebo and treated local councils for all collisions together and each type of accident separately. In every panel, the black line is our treatment effect as seen in Figures 4 and 5, i.e., the gap in accident rates between Scotland and synthetic Scotland. The gray lines instead represent the gaps associated with each of the runs of the placebo test. In the pre-reform period, the difference between Scotland and synthetic Scotland falls in the middle of the placebo tests. This continues to be the case even after the passing of the new legislation. Evaluated against the distribution of the gaps for the placebo districts, therefore, the gap for Scotland does not appear to be unusual. We thus conclude that the 2014 DDL reform had no effect on accident rates. This conclusion differs from the results found by Dee (2001) and Eisenberg (2003) for fatal crashes in the US. It is in line, instead, with the findings reported in Carpenter (2004) for drink driving among individuals around the legal drinking age. It is also in line with the results shown by Haghpanahan et al. (2019) and Cooper, Gehrsitz, and McIntyre (forthcoming) on the same 2014 Scottish reform we study. In comparison with the latter two studies, however, the inferential evidence from our evaluation, however, is likely more credible for it is drawn from finer geographic details and is based on more data and more careful research designs.

B. Heterogeneity

The null results in the benchmark analysis above may mask considerable effect heterogeneity. The reform may have an impact in specific cases where alcohol is likely to be most consumed. Age and context are known to be key alcohol reinforcers, which may affect brain activities and trigger drink driving (Zironi et al., 2006; Corbit, Nie, and Janak, 2012). We therefore distinguish accidents by the timing of when they occur and by drivers' age. We also look for differential impacts by gender and by the number of vehicles involved in crashes. For each of the next four pieces of analysis, we use our preferred approach based on the synthetic control method, re-estimating the control group every time for each outcome and subgroup. To summarize the effects, we present the estimated gap in the road accident rates that compares Scotland with its synthetic counterpart along with the corresponding placebo gap effects. For all comparisons, we remove the local authority districts with a pre-treatment MSPE that is more than two times the corresponding MSPE found with the synthetic control.

[25] This is a conservative cutoff that discards districts with extreme values of pre-DDL reduction MSPE for which the synthetic control method would be ill-advised. In Appendix Figure A.4 and Appendix Figure A.5, we report the results found when we impose more lenient rules, i.e., when we discard districts with pre-reform MSPE 10 or 5 times higher than synthetic Scotland. The results from those exercises strongly confirm what we have in the text.

Timing of Accidents — We first examine the impact of the tighter BAC limit at different hours of the day and different days of the week when alcohol consumption is more or less likely to play a role (Francesconi and James, 2019). The estimates in Appendix Figure A.7 divide the observations into those during the day, 8:00am till 8:00pm, and those in the night, 8:00pm till 8:00am in the following morning. There is no evidence of a difference in the impact of the new limit by time of the day. Neither does there appear to be an impact of the DDL on road accidents that happen at weekends as shown in Appendix Figure A.8. Defining day/night times and weekends differently yields results of much the same order of magnitude and statistical significance.

Age of Drivers — We next consider the possibility that drinking varies by age (Naimi et al., 2003), and so does risk taking behavior in drink driving (Levitt and Porter, 2001). Both channels may lead to heterogeneous effects of the reform. Estimates for those accidents that involved at least one driver aged 18–25, 18–30, and 50 years or more are shown in Appendix Figure A.9. We cannot detect any impact of the lower Scottish BAC limit on motor vehicle crashes across the three age groups. Distinguishing finer age groups and including individuals aged 30–50 do not change the results.

Gender of Drivers — Men take more risks when driving and drink more than women (Holmila and Raitasalo 2005; Rhodes and Pivik, 2011), although recent research documents increased drinking among women (White et al., 2015; Wilsnack et al., 2018). We thus estimate the impact of the new DDL on road accidents by sex. Appendix Figure A.10 reports the effect on collisions with the involvement of at least one man (top panel) or at least one woman (bottom panel). We find no impact for either sex. Restricting attention to crashes in which only men were involved leads to the same result.

Number of Vehicles Involved in Accidents — Multiple vehicle crashes could have larger externalities and may affect individuals who were sober at the time of the accident. Therefore, we analyze whether the effect of the BAC reduction was different in one vehicle accidents as opposed to multiple vehicle crashes. The results in Appendix Figure A.11 document a zero effect of the reform on both types of collisions.

In sum, we detect no effect of the stricter BAC Scottish law on road traffic accidents, even in circumstances that are more likely to be associated with greater alcohol consumption (such as weekends and multiple vehicle crashes) or among individuals who may experience heavier drinking (such as young adults and men).

C. Robustness Checks

We present a number of exercises to check whether the finding that the reform had no impact is an artifact of either the definition of the dependent variable, functional forms used in estimation,

estimation approaches, or methods of inference.[26]

New Definitions of the Dependent Variable — In the benchmark analysis, our outcomes are based on the number of monthly accidents per 1,000 registered vehicles in each local authority district. We redefine them using three variants, that is, the number of monthly accidents per 10,000 of the population, the number of monthly accidents per 10,000 of the adult population, and the number of monthly accidents per kilometer of road in the local council. Each of these alternative definitions aims to identify possibly different dimensions of the 'populations' at risk of experiencing a car crash. All three measures produce results similar to the benchmark estimates shown above.

Alternative Specifications of the Synthetic Control Method — In the baseline synthetic control analysis, we use covariates as well as the outcome variable to create our synthetic controls. We perform two checks of this approach. First, we create synthetic control groups that only match on outcomes as proposed by Botosaru and Ferman (2019). Second, we use demeaned data that can help improve the quality of the match (Ferman and Pinto, 2019). Both checks emphatically uphold the benchmark estimates.

Count Data Models — An additional strategy is to estimate the impact of the reform using count data models, in which our new dependent variable is the number of collisions in a given month. When estimating such models, we control for either registered vehicles (or population) by local authority district to account for variation in size across local councils. The estimates found with this alternative approach reveal the same null results found in the benchmark analysis.

Randomization Inference — One concern is that the reform occurred at the constituent country level, and standard errors should then be clustered at this level rather than at the local council level as we do in the benchmark analysis. Following the approach discussed by Barrios et al. (2012) and Young (2019), we re-estimate our baseline results using Fisher's randomization test (Fisher, 1935). We should stress that, unlike Young (2019), our issue is not a systematic bias in favor of finding significant results, since so far we have not found significant effects of the reform. The goal of this exercise therefore is to check whether alternative inference procedures lead us to a different conclusion. Perhaps unsurprisingly, this exercise does not change the conclusion of our baseline results.[27]

Event-Study Design — A different concern is that there might be unobservables that affect accident rates and, possibly, even the enactment of the DDL reform. One of such unobservables is alcohol

[26] All the results that are not presented in the text or the Online Appendix are available upon request.

[27] A related, but different, issue is the possibility that the error term, ε_{cm}, in the DD model (1) is serially correlated, especially when we have fewer geographic units of observation than time periods. Although this is not the case for the estimates reported in Tables 2 and 3, re-estimating those models using a Prais-Winsten council specific AR(1) error structure does not lead to different results. In the subsequent analysis, whenever the time dimension exceeds the cross-sectional geographic dimension, we will fit DD models that are corrected for first-order serially correlated residuals using the Prais–Winsten transformed regression estimator. All our results, however, are not sensitive to this correction.

abuse. In the benchmark DD framework, we account for this possibility by including a large set of controls, while group-specific linear trends and month dummies are meant to capture pre-reform trends and seasonality in collision rates, which could be related to alcohol abuse. The presence of differential pre-reform trends may be taken as evidence against strict exogeneity of the reform, which would justify our approach. But we could fail to detect pre-reform trends not only if they actually do not exist but also if there is not enough statistical power to detect them.

In this exercise, we follow the linear panel event-study design proposed by Freyaldenhoven, Hansen, and Shapiro (2019), in which causal inference is valid even when there are pre-reform trends in the outcome variable. According to this approach, the effect of the lower BAC limit can be estimated by a two-stage least squares (2SLS) regression of the outcome on the reform and observed covariates as in (1), with leads of the policy serving as excluded instruments. Specifically, we assume that the local unemployment rate responds to alcohol abuse among drivers, but plausibly not to legal changes in BAC levels. A large empirical literature documents strong positive effects of ethanol intake on work days lost due to industrial injuries (Ohsfeldt and Morrisey, 1997) and finds that alcohol abuse results in reduced employment and increased unemployment (Terza, 2002; Henkel, 2011). Therefore, instead of using the fraction of working-age individuals in each local authority district claiming Job Seeker's Allowance as a control variable, as we have done in the benchmark analysis, we look at its dynamics around the DDL reduction and use this to infer the dynamics of driving while intoxicated.

We summarize the 2SLS results in Figure 7, where monthly level information is aggregated at the quarterly level for graphical convenience.[28] Four or more quarters before the reform, Scotland had a higher motor vehicle collision rate than the rest of Britain, driven exclusively by slight injury accidents. But in the last three quarters pre-reform, the difference was small and statistically insignificant. After the new legislation, we continue to find no differential in car crash rates, regardless of the type of accident or the time horizon. These results bolster the zero-effect evidence from the benchmark estimates. The results from further sensitivity analysis, in which we exclude or include local unemployment rate as a proxy for alcohol involvement, are in Appendix Figures A.12 and A.13 and reiterate these findings.

Alternative Definition of Drink Drive Accidents — The previous exercise emphasizes the importance of alcohol abuse. As mentioned in Section 3, the information on alcohol involvement in the RAD records is incomplete, since police officers may not always breathalyze all drivers at the roadside (e.g., those who died) and some drivers may depart the accident scene before the arrival of the police in hit-and-run cases. To address this issue, we use new data compiled by the DfT and, like RAD records, derived in part from STATS19 forms. These are supplemented with detailed information on hit-and-run accidents and toxicology data on fatalities from coroners in England and Wales and public prosecutors (or procurators fiscal) in Scotland.[29] These new drink drive data, however, are

[28]The estimates found using monthly data are qualitatively identical and thus not shown.

[29]In this new data, the definition of a drink drive accident not only is based, as before, on reporting a positive roadside

coarser than those used in the benchmark analysis in two important dimensions: they come at an annual frequency from 2009 to 2016 (not permitting us to define accident rates at the monthly level) and at a less granular geographic detail (i.e., instead of 378 local councils, we only have nine English regions, the whole of Wales and the whole of Scotland). This means we cannot rely on spatial RD models or synthetic control methods.

We then estimate DD models that follow as closely as possible the baseline specification, accounting for differences in annual regional average temperature range, population density, road length, proportion of residents with no qualification, proportion of individuals with bad or very bad general health, median hours worked per week, Job Seeker's Allowance rate, and the number of licensed alcohol premises. Appendix Table A.1 presents the results. We find a small and negative overall impact of the reform on all drink drive related accidents. The point estimate relative to the mean is around 2% and is not statistically significant. When group-specific annual trends are included (column (c) onwards), the estimates change sign but remain always statistically insignificant. Serious and slight injury accidents follow the same pattern. Contrary to what we may expect, the estimates for fatal drink drive collisions are positive and significant even when controls are included (panel B, columns (a) and (b)). However, when we account for differential trends (column (d)) and region fixed effects (columns (e) and (f)), the point estimates become statistically indistinguishable from zero.

Accidents with Positive Breath Testing Only — Another related check is to repeat the benchmark analysis but only on motor vehicle crashes in which at least one of the drivers tested positive (i.e., above the legal limit) or refused to be tested by accident type. The DD and spatial RD estimates are reported in Appendix Table A.2, while the synthetic control results are summarized in Appendix Figure A.14. Irrespective of accident severity and estimation method, we find no evidence of an impact of the stricter limit.

Matching and Synthetic Control (MASC) Approach — To deal with the potential interpolation bias induced by the synthetic control method mentioned in Section 3, we estimate MASC models as formulated by Kellogg et al. (2019). Together with the estimates shown in Figures 4 and 5, Appendix Figure A.15 adds the district-specific accident rates found for MASC Scotland. Across all five crash types, the MASC estimates track Scotland's accident rate trajectories extremely well over the entire pre-intervention period.

In line with the synthetic control results, the MASC Scotland estimates also confirm that the stricter Scottish DDL had no impact on road traffic collisions. This is clearly illustrated in Figure 8, where we plot the difference between Scotland and synthetic Scotland, as in Figure 6, as well as the difference between Scotland and MASC Scotland for each accident type. The figure reveals that the two estimators not only perform equally well in replicating Scotland's pre-reform crash rates,

breath test or refusing to give a breath test when requested by the police, but also includes cases of individuals who died and, within 12 hours of the accident, were found to be above the legal limits.

but are also broadly equivalent in what they capture after the enactment of the new legislation.

To sum up, the evidence reported here decisively supports the benchmark estimates according to which there was no impact of the new Scottish DDL on all types of road accidents, including those very precisely defined as alcohol related, and even if we focus only on the subsample of crashes in which the drivers tested positive. The null result emerges also when we allow for randomization inference, when we use a linear panel event-study design, and when we combine matching and synthetic control estimators.

5. Why Was the DDL Reform Ineffective?

Addressing this question is crucial for both our understanding of human behavior and improving policy design. To find answers, we resort to the insights of the market model of crime à la Becker (1968) and Ehrlich (1973), which yields an equilibrium where marginal costs equal marginal benefits of drink driving. Put differently, equilibrium in this market hinges on the notion of equality between the slope of an opportunity boundary (the production transformation curve of the composite good which individuals care about when they decide to drink and drive as opposed to driving sober, drinking without driving, or using other means of transportation) and the slope of an indifference curve, which embeds individual preferences and the probability of being caught driving under the influence.

Economic incentives affect the drink drive decision in a number of direct and indirect channels (Draca and Machin, 2015). We explore two of such channels that might have affected the market equilibrium through the DDL reform. Among the direct incentives, which could have changed the net return of engaging in drink driving, we consider the availability and prices of buses and taxis. These represent the most important means of transportation other than one's own vehicle. Among the indirect incentives, which operate through deterrence and incapacitation, we analyze measures of enforcement, such as the number of police officers, breath tests unrelated to motor vehicle collisions, and arrests and convictions for other crimes related to drink driving.

A. Alternative Means of Transportation

Greater provision and/or lower fares of means of transport other than own vehicles increase the opportunity cost of driving under the influence. An increase in the opportunity cost might encourage people to shy away from drink driving. No change in availability or prices, instead, will provide a possible explanation for why the reform did not save lives.

Taxi Availability — To gauge the extent of this alternative supply of transportation means, we combine information on taxis and private hire vehicles (which we refer to as 'taxis' or 'cabs'),[30]

[30] Taxis are available for immediate hire and can be hailed in the street or pre-booked. Private hire vehicles (or minicabs), instead, must be pre-booked, cannot ply for hire, and cannot use taxi ranks.

and use two separate measures of availability, that is, the number of driver licenses and the number of vehicle licenses.[31] The data are collected every two years from 2009 to 2015 by the DfT and aggregated at the local authority highway level, of which there are 314 in England and Wales and 32 in Scotland.

Appendix Figure A.16 shows the means of the two types of license per 10,000 heads of population averaged across local authorities. Regardless of whether we consider drivers or vehicles, there were more taxis per capita available in Scotland relative to the rest of Britain. For Scotland, however, between 2009 and 2013, we observe a slow decline in both types of license, whereas in England and Wales the average number of licenses was stable. After the stricter DDL, cab driver licenses continued to fall in Scotland, while remaining unchanged in England and Wales; license rates instead did not change in post-reform Scotland, but increased south of the border.

To examine the impact of the reform on cab availability, we estimate DD models as in (1) separately for each type of license and report the results in Table 4. We find the tighter BAC law led to a reduction in availability in Scotland of about 7–9%, with or without controls and even after accounting for local authority fixed effects (columns (a)–(c)). But for both license types, the inclusion of group-specific trends wipes out the differences between Scottish districts and their English and Welsh counterparts, yielding small and statistically insignificant treatment effect estimates (columns (d) and (e)). These no-impact results are corroborated by the estimates found with both the spatial RD framework in Appendix Table A.3 and the synthetic control approach in Appendix Figure A.17.

Taxi Tariffs — Information on taxi tariffs comes from the Private Hire and Taxi Monthly, the official newspaper of the UK National Private Hire Association.[32] The tariffs recorded in the data refer to the maximum that can be charged in a given council and are grouped by average day rates and average night rates. Our main results are based on monthly fare average tariffs for 2-mile journeys at the local authority district level from January 2009 up to November 2016. We also analyze four other categories, distinguishing the average cost of hailing a cab (or minimum fare), the average cost of 1- and 10-mile journeys, and the mean charge per mile travelled after the initial pull-off distance (or running mile fare). For such four categories, however, we can only use monthly fare averages at the regional level, since the information at the geographically more disaggregated council level is not available.

Table 5 reports the DD and the spatial RD results. The DD estimates point to a modest increase of about 2.5% in taxi tariffs (columns (a) and (b)), although this impact becomes smaller and significant only at the 10% level when group-specific trends are accounted for (columns (c)–(e)). The spatial RD estimates reveal a larger increase of 6%, but at the margin of statistical significance and only for councils that are 50km from the border. The estimates found with greater bandwidths

[31] Driver licenses are issued to the driver, while vehicle licenses are issued to the owner of the cab (who may be the same as the driver, or another individual, or a company).

[32] See <https://www.phtm.co.uk/taxi-fares-league-tables>.

are smaller and statistically indistinguishable from zero. The synthetic control placebos in Figure 9 show the reform had no effect on taxi tariffs. This last result uphold the graphical patterns presented in Appendix Figures A.18 and A.19. Finally, the DD results in Appendix Table A.4 decidedly confirm the null effect on the other four types of tariffs at the regional level.

Bus Availability — We examine data from the DfT Public Service Vehicle Survey, which are available annually from 2004/05 to 2016/17 for each of the three constituent countries of Great Britain (England, Wales, and Scotland). With this high level of aggregation (in time and space), we can only rely on difference-in-difference models. We have three different measures of bus usage, i.e., the number of bus journeys per capita, bus miles per capita, and average bus occupancy.

Appendix Figure A.20 shows a slight downward trend in the number of bus journeys in all three constituent countries, with the trends not changing after the reform. Vehicle miles also decline in all three countries since 2009, although this fall stops in Scotland from 2012 but continues in England and Wales. Average occupancy, which is calculated by dividing passenger miles by vehicle miles, has increased in England and Wales over the whole period and in Scotland up until 2010, when occupancy began waning. The DD estimates in Appendix Table A.5 reveal no evidence that the 2014 reform had an impact on the three measures. We only detect a weak treatment effect increase in bus miles traveled per head, but this impact was economically negligible, representing merely a 1% increase, and statistically significant only at the 10% level.[33]

Bus Fares — Local bus fare indices (both in current and constant prices) are published by the DfT from 2004/05 to 2016/17 at the country level. Although bus fares increased in all three countries throughout the sample period (see Appendix Figure A.21), Appendix Table A.6 unambiguously indicates that the reform led to no differentials in bus prices, whether we consider fares in current or constant prices and irrespective of the definition of the post-reform period.

Putting together the results so far, we conclude that, after the enactment of the 0.05 BAC law in Scotland, Scottish drinkers did not have greater opportunity costs of driving their own vehicles as bus fares and taxi tariffs did not fall, and bus and taxi availability did not go up. The reduction in cab provision and the increase in taxi tariffs, which we find in some specifications, may even suggest a possible increase in incentives in driving while intoxicated post intervention. The unavailability of (cheaper) alternative means of transportation therefore may be one of the channels that frustrated the efficacy of the reform.

B. Enforcement of the New Limit

In a market model of crime, the optimal amount of enforcement depends, among other things, on the cost of catching and convicting offenders, the responses of offenders to changes in enforcement,

[33]Since we have annual (April to March) data, the post-reform period could be defined in reference either to 2015/16 only or to both 2014/15 and 2015/16. The results in Appendix Table A.5 show that this distinction makes no difference to the results.

and the nature of punishments (Becker, 1968; Mookherjee and Png, 1994). Punishment (criminal or otherwise) remained unchanged everywhere in the UK after the stricter DDL. Also, there was no explicit anti-drink drive crackdown in Scotland (such as "hot spot" policing in the most crime-prone locations or random checking across widely spread locations) that might have affected lawbreakers in a predictable way, e.g., switching to unpoliced roads.[34]

We focus on police numbers, breath tests unrelated to car crashes (which are thus not part of the RAD STATS19 records examined in Section 4), as well as drink drive arrests and convictions unrelated to road accidents that are not recorded in the RAD data. Against the backdrop of the 2007/08 global financial crisis and the austerity program launched by the UK government after the 2010 elections, these outcomes should be seen as measures of the extent of law enforcement and deployment of scarce resources.

Police Numbers — Due to restructuring of the police force in Scotland and to changes in recording police activities around the reform, we can only examine police numbers overall, and not the number of police officers deployed in specific activities, such as traffic duties, or their hours worked in such activities.[35] From the Police Officer Quarterly Strength Statistics, we have annual information on police numbers for the 13 police forces in Scotland between 2013 and 2016, while for the 43 English and Welsh police authorities we obtain the same information from the Home Office Police Workforce.

Appendix Figure A.22 shows the trends in the average number of police per 100,000 of the population. It is worth keeping in mind that the sample period overlaps with the austerity program introduced by the central government in response to the 2008/09 financial crisis, which included cuts in police spending nationally. We observe a slightly greater decline in police officers per capita prior to the reform in England and Wales relative to Scotland. Appendix Table A.7 shows the DD results across a number of specifications. The treatment effect estimate without controls and trends is positive and reveals an impact of 8 more officers per 100,000 of the population, corresponding to a 2.5% increase. But the inclusion of group specific annual trends leads to a small and insignificant impact, while including police force area fixed effects implies a sign reversal, although the impact remains statistically insignificant and small. As shown in Figure 10, this null result is strongly backed up by the synthetic control approach.

Breath Testing — Another, perhaps more direct, measure of enforcement can be inferred from the number of breath tests actually carried out at the roadside.[36] These tests include not only

[34] Haghpanahan et al. (2019) conjecture that the lack of an impact on road traffic accidents was due to the lack of enforcement of the legislative change in Scotland. They, however, do not document this claim. The importance of enforcement in a similar context is shown by Banerjee et al. (2019), who analyze the effect of an anti-drink driving campaign in the Indian state of Rajasthan which was implemented in a randomized fashion. They find that random checking was highly effective, reducing night accidents by 17% and night deaths by 25%. See also the discussions in Draca and Machin (2015) and Chalfin and McCrary (2017).

[35] In the Appendix we discuss the main changes in Police Scotland and the recording of activities by all police forces which do not make possible analyses other than the one we perform.

[36] Breath testing is one of the key policies recommended by the WHO to reduce the harmful use of alcohol (World

those administered after of a car crash, which we analyzed in Section 4, but also those unrelated to collisions. Although random breath testing is not permitted, the police across Britain do not need to give a specific reason to stop a vehicle. To breathalyze a driver, police officers must have a reasonable suspicion that the driver has consumed alcohol. During a routine stop, this can be judged for instance by smelling alcohol or whether the driver appears intoxicated. Even without suspicion of alcohol intake, breathalyzation can occur when the driver has committed a traffic violation.

For England and Wales, the data are published by the DfT and are collected using digital breath testing devices by each of the 43 police forces. For Scotland, we do not have access to the same type of data. We instead use the only available information that is published by the Parliamentary Advisory Council for Transport Safety. Normally, there are two periods a year when Police Scotland collect and release data on breath tests, namely, during the summer and the winter festive season (Christmas and New Year). We have data from 2013 to 2016. The data collection can last either a fortnight or four weeks.[37] To make the data as comparable as possible, we take the Scottish collection periods and select the equivalent time windows from the English and Welsh data. Two week campaigns are scaled up to their four week equivalents.

We analyze two outcomes, the number of breath tests administered per 1,000 population and the proportion of positive tests. Appendix Figure A.23 shows the breath test rates in Scotland were about twice as high as in England and Wales both before and after the 2014 reform. Everywhere, and irrespective of the tighter DDL, police forces seem to have peaks of breath testing checks during the end-of-year festive season. The DD results in Appendix Table A.8 reveal that, despite the differences in levels by group, there is no statistically significant treatment effect estimate for both outcomes. In fact, when we include controls and group specific trends, we find that Police Scotland administered 20% fewer tests after the reform. This is also confirmed when we further control for police force area fixed effects. The synthetic control estimates shown in Appendix Figure A.24 reveal however that the gaps in both outcomes for Scotland and the placebos are inexistent. Repeating the analysis using the number of breath tests per 1,000 drivers leads to the same conclusions.

Drink Drive Arrests Unrelated to Road Accidents — In the UK, data on all drink driving offenses are not published on a monthly basis at the local (council or regional) level. To gather such data, we therefore contacted every police force in Great Britain submitting a Freedom of Information (FOI) request.[38] The recording procedures of drink driving offenses differ slightly across constituent countries. In England and Wales, drink drive arrests are recorded, although drink drive offenses are not logged as a specific crime per se. In Scotland, instead, arrests are not collected, but Police Scotland register the number of reported and detected crimes together with statistics regarding

Health Organization, 2010) and endorsed by campaigners and charity organizations working to prevent and cut alcohol-related harm (see <https://www.alcohol-focus-scotland.org.uk/campaigns-policy/>).

[37] Specifically, for the summer campaign, we have data on four weeks in June 2013 and two weeks in June 2014–2016, while for the winter festive season we have data on four weeks in December between 2013 and 2016.

[38] For more details on the Freedom of Information Act and representativeness of samples collected using FOI requests, see Clifton-Sprigg, James, and Vujic (2020).

drink driving offenses.[39] For the period going from January 2010 to December 2016, we have data for the whole of Scotland from Police Scotland, disaggregated into 13 police force areas, and for 13 police forces in England and Wales. We also have data from nine other English and Welsh forces but over shorter time frames.[40] Our outcome variable is the monthly regional crime rate, defined as the number of drink drive arrests or offenses divided by 100,000 heads of population in each of the correspondent police force areas. Given the data are at the police force level and are geographically coarser than those based on local councils, there is not much scope to perform the analysis using the spatial RD design.

Table 6 displays the DD results. All the point estimates suggest a lessening in drink drive crime rates in Scotland but none are statistically significant. Figure 11 displays police force area specific arrest rates for drink driving for Scotland and synthetic Scotland. While there appears to be a reduction in the arrest rate immediately after the introduction of the 2014 reform, the two series (for Scotland and synthetic Scotland) begin to overlap again after a couple of months.[41] This is clearly reiterated in Figure 12, which presents the gaps in arrest rates for Scotland and the placebos.

Drink Drive Convictions Unrelated to Road Accidents — Data on convictions, made available from the Criminal Proceedings in Scotland and the Ministry of Justice in England and Wales, are only published at an annual frequency (from 2008 to 2016) and at the country level (i.e., Scotland on one hand and England and Wales on the other). This means we cannot perform any analysis using spatial RD models or synthetic control methods. Nonetheless, the lack of an effect on arrest rates found earlier emerges also in the case of convictions. Appendix Figure A.26 shows the annual conviction rates for driving under the influence, computed per 10,000 population. From 2008 to 2011, England and Wales matched closely to Scotland, with conviction rates falling in both constituent countries at roughly the same pace. From 2012, the decline slowed down in England and Wales but continued in Scotland. However, after the 2014 Scottish DDL reform, there were no changes in conviction rates between the two sets of countries. The DD results shown in Appendix Table A.9 confirm this pattern of no impact.

In spite of the challenges imposed by some of the data, the evidence on enforcement is compelling.

[39] Appendix Figure A.25 in the Supplementary Material of the Online Appendix contains the response from Police Scotland for the request regarding arrests for drink driving.

[40] The English/Welsh Police forces that provided the full data through the FOI requests are: Cambridgeshire, Cheshire, Cumbria, Hampshire, Hertfordshire, Kent, Lancashire, Metropolitan Police (Greater London), Norfolk, South Yorkshire, Staffordshire, Surrey, West Yorkshire. The forces that provided partial time data are: North Wales (January 2012–December 2016); Dyfed–Powys (January 2011–-December 2016); Thames Valley (June 2012–-December 2016); West Midlands (August 2010–-December 2016); Bedfordshire (September 2016-–December 2016); Devon and Cornwall (April 2012–December 2016); Dorset (May 2015–December 2016); Cleveland (January 2010–December 2012 and April 2013–December 2016); Leicestershire (April 2010-–December 2016. Excluding from estimation the data from these nine police forces yields results similar to those presented in the text. Such robustness estimates are available from the authors.

[41] The initial impact may be driven by an increase in the number of overtime shifts worked by Scottish police and road traffic officers. Without data on police hours, however, we cannot test this possibility directly. But, as argued by Draca, Machin, and Witt (2011), who analyze the increased security presence following the terrorist attacks that hit central London in July 2005, this is likely to be only a temporary strategy, which — as shown above — was not accompanied by a longer run expansion in the number of police officers per capita.

Compared to the rest of Britain, Scotland did not experience a relative increase in police officers per capita and Scottish drivers did not face greater odds of being breath tested after the stricter 0.05 BAC limit was imposed in December 2014. Similarly, neither arrest rates nor conviction rates related to drink drive offenses that did not end up in a motor vehicle collision show differences in Scotland as opposed the rest of Britain after the legislative change. Everything else equal, therefore, potential offenders in Scotland did not see a significant change in the opportunity cost of driving while intoxicated, as their expected marginal cost of punishment remained unaltered.

This evidence and the very absence of an anti-drink drive crackdown suggest that the inefficacy of the DDL reform to save lives may be due to lack of enforcement. This adds to the mechanism linked to the availability and prices of alternative means of transportation documented earlier. Although it is hard to determine which of the two channels played a bigger role, we observe they both unambiguously worked in the same direction.

6. Unintended Consequences and Spillover Effects

Section 4 shows that the 2014 DDL reform was ineffective in reducing all sorts of motor vehicle accidents on Scottish roads. The reform, however, might have triggered unintended (or serendipitous) consequences on other important domains of behavior, such as attitudes, car usage, alcohol intake, food consumption, and smoking. It might have also generated spillovers on offenses and crimes other than drink driving (including speeding, illicit drug usage, robbery, and sexual offenses) and unleashed aggregate responses from the alcohol and car industries. Here we examine all of these dimensions.

A. Public Attitudes Toward Drink Driving

Even though police enforcement and alternative means of transportation did not adjust to enable an effective implementation of the reform, lawmakers may have counted on the near-zero-tolerance BAC threshold to act as a deterrent to driving while intoxicated. It is often argued that for the law to be an effective deterrent, the actual and perceived risk of detection and punishment must be sufficiently high (Sah, 1991; North, 2010).

In what follows, we focus on the way in which the new limit affected public sentiment on the acceptability of drink driving and the risk of being caught and punished. To make these assessments, we use repeated cross-sectional data from individual questionnaires collected in the British Social Attitudes Surveys between 2009 and 2016. We examine two perceptions. The first is in relation to the statement "If anyone has drunk any alcohol they should not drive", and the second refers to "Most people don't know how much alcohol they can drink before being over the legal drink drive limit". Answers to both questions are recorded on a five-item Likert scale. Appendix Figure A.27 plots the proportion of respondents who agree or strongly agree with each statement over time. Prior to the reform, a larger fraction of the Scottish public already believed that no alcohol should be consumed before driving. After the reform, the difference between Scottish respondents and their English and Welsh counterparts further increased. Differences in the perceived knowledge of the legal limit were instead negligible pre-reform, and remained contained also after 2014.

The DD estimates in Table 7 show that the reform did not affect the public's perceptions about DDL knowledge (panel B). The reform however increased attitudes against drink driving among Scottish respondents by 7–9 percentage points (panel A). With a pre-reform mean of 90%, this effectively implied a zero-tolerance sentiment toward drink driving among the Scottish public.

The new law therefore changed public attitudes, some at least. The public (including potential lawbreakers) increased their perception of a greater expected marginal cost of punishment, even though actual sanctions remained unaltered. But not even a unanimous perception that drink driving was harmful brought about deterrence adequately. The results on motor vehicle collisions and usage of alternative means of transportation suggest deterrence through the change of attitudes did not work.[42]

B. Own Motor Vehicle's Usage

Had a heightened attitude against drink driving translated into stronger perceptions of the risk of flouting the new limit, then it is plausible that Scots drove less. This should be even more so at night or during the weekend, when more alcohol is consumed, or among young males, who engage in ethanol seeking behavior more than others (Francesconi and James, 2019).

To test this possible response, we use annual DfT data on vehicle miles travelled in a year per person by local authority districts from 2009 to 2016. In Scotland and in the rest of Britain, the trends were similar and slightly downward sloping (Appendix Figure A.28). The DD and spatial RD estimates are in Appendix Tables A.10 and A.11, respectively, while the synthetic control results, which plot the gaps for Scotland and synthetic Scotland as opposed to the gaps for Scotland and the placebos in the rest of Britain, are in Appendix Figure A.29. The results from all three methods indicate clearly that the 2014 reform led to no difference in miles travelled by Scottish drivers as opposed to their English and Welsh counterparts.

We also check whether these patterns are confirmed with individual data from the UK Time Use Survey (UK-TUS), conducted between April 2014 and December 2015 on about 9,500 individuals aged 8 years and over.[43] The dependent variable is the number of minutes spent driving between 6 p.m. and midnight in any day of the week or during the weekend. Regardless of which day we focus on, the difference-in-difference estimates in Appendix Table A.12 are always quantitatively small and statistically insignificant. They are also small and insignificant if we consider only male

[42]In future research, it would be interesting to explore the link between drink driving and perceptions as a two-step process, as suggested by the criminology literature (e.g., Pogarsky, Piquero, and Paternoster, 2004). In the first, the new DDL ix expected to affect perceptions, while in the second perceptions affect criminal behavior. This analysis would rely on data that are currently unavailable for Britain.

[43]Respondents filled up two 24-hour diaries, one completed on a weekday and one on a weekend day. This gives us a total of about 16,500 diaries. As usual in time use surveys, each day is broken down into ten minute sections with activity, co-presence, and location recorded. More details are in the Online Appendix.

drivers, or drivers aged 18–30. The results are robust to changing the time blocks (e.g., 6 p.m. to 3 a.m., or 9 p.m. to 3 a.m., or midnight to 6 a.m.), or restricting weekends to be only Fridays and Saturdays or only Saturdays and Sundays, or altering age groups.

It is apparent that Scots did not drive less compared to their English and Welsh counterparts as a result of the stricter BAC limit. This finding bolsters the evidence in Section 5, where we document that Scots did not face a greater opportunity cost of driving their own vehicle.

C. Alcohol Consumption

If would-be offenders perceived the reduced legal limit as a factor that lowered the expected utility of drink driving, they might have cut alcohol intake before driving, even though we do not see lower road accident rates. We shall examine this conjecture from the alcohol industry's viewpoint in a later subsection. Here, we take the individual consumer's perspective and analyze data from the Health Surveys of England and the Scottish Health Surveys over the period 2008–2016.

Both data sources ask similar questions on alcohol consumption. One is the number of alcohol units drunk on the heaviest day during the preceding week. This enables us to construct a second measure, namely, drinking 10+ units on the heaviest day, which we take as a proxy for binge drinking (Francesconi and James, 2019). A third measure is the number of days in which individuals drank over the past week, and the last is the number of alcohol units usually drunk per week.

Appendix Figure A.30 displays the trends by country. Across all definitions, alcohol consumption has been waning over the sample period in both countries, although the decline is slightly more pronounced for England. Table 8 presents the corresponding DD treatment effect estimates. Controlling for a standard set of socioeconomic variables, we find that the stricter DDL law led to a modest increase in all indicators of alcohol intake among Scots, except for binging. But controlling further for country specific trends eliminates any differentials in alcohol consumption along all four definitions among individuals north and south of the border. We also find no evidence of a differential impact by gender, age, or education. These results are strengthened by the time diary estimates in Appendix Table A.13, which show that Scottish people did not change the time spent in pubs and restaurants after the passage of the 2014 reform, either during the week or at weekends, for men or women, and for the young or less young.

Not only was the stricter BAC rule unable to save lives on the road, it also did not affect ethanol seeking behavior, notwithstanding its effect on public attitudes with regard to drink driving.[44] Seen from the perspective of the Becker-Murphy model, the lack of an impact on both alcohol intake and accidents could be driven by the fact that the perceived increase in the price of driving while intoxicated due to the reform was too small to give rise to a substantial reduction in drink drive consumption and consumption capital, i.e., the number of drink drive events experienced by

[44] The zero effect on consumption may reflect reporting error, since the adjustment in alcohol intake could have been inconspicuous, although changes and re-calibrations around defaults are known to be salient (e.g., Beshears et al., 2009).

consumers over time. This was likely reinforced by learning that the new limit was not enforced and that more (or cheaper) travelling options were not available to drinkers.

D. Healthy Eating and Smoking

In the Introduction, we touched upon the poor record of Scotland in terms of life expectancy relative to the rest of Europe, and how these featured prominently in the policy discussions leading up to the 2014 reform. Health information dissemination is known to have powerful effects on health behavior and other outcomes (e.g., Viscusi, 1990; Miguel and Kremer, 2004; Cawley and Ruhm, 2012; Junior and Rasul, 2020). The detrimental consequences of alcohol excess advertised in the pre-intervention campaign could have led consumers to adopt healthier behaviors, such as improving diets and reducing (or even giving up) smoking (de Walque, 2010; Capacci and Mazzocchi, 2011).

We explore these potential influences with the same individual-level data we used for alcohol consumption, i.e., the Health Surveys of England and the Scottish Health Surveys, 2008–2016. We consider two outcomes for healthy eating, namely the likelihood that an individual consumes at least one portion of fruit and vegetables a day and the likelihood of eating 5 or more portions of fruit and vegetables daily (which corresponds to the recommended diet by the WHO and the UK National Health Service),[45] and two for smoking, i.e., the probability of smoking at the time of the survey and the number of cigarettes smoked in a day.

Appendix Table A.14 shows the difference-in-difference estimates. The probability of eating either one or 5+ fruit and vegetables a day was not affected by the reform. Likewise, smoking habits did not change: Scots were not less likely to smoke nor did they smoke fewer cigarettes. This latter result emerges also when we condition on the subsample of smokers. Such findings, which can be attributed to the lack of any information targeting by policy makers, are consistent with the idea that the mere provision of information might not matter much for health when other forces are at play, such as externalities, limited attention, or credibility (Bennett, Naqvi, and Schmidt, 2018).

E. Shifts to Other Offenses and Criminal Activities

Scottish drinkers may have responded to the stricter DDL with the decision to drink drive but be extra careful with speed limits and other traffic violations, such as driving without seat belts on or while using a mobile phone, minimizing the risk of collision or police suspicion.

It is also possible that some individuals pre-committed not to drive, relying for instance on car sharing with sober friends in their social outings.[46] In doing so, however, they might have allowed themselves to drink more and, being intoxicated, they could have gone on and committed criminal offenses other than drink driving. This behavior is consistent with the notion of moral

[45] See <https://www.nhs.uk/live-well/eat-well/why-5-a-day>.

[46] Using time diary data from the UK-TUS, we find no evidence of an impact of the DDL reform on the time spent by Scots as car passengers, for the whole population, during any evening/night of the week, at weekend nights, and among young males, although we cannot establish if the passenger was drunk and the driver sober. The results of this analysis are available on request.

self-licensing, according to which the decision of choosing a legal activity (i.e., not drink driving by abstaining from driving but not from drinking) may give people the moral license to behave illegally after drinking (Sachdeva, Iliev, and Medin, 2009; Blanken, van de Ven, and Zeelenberg, 2015).[47] The same behavior could also be explained either by a temptation model à la Gul and Pesendorfer (2001), where individuals are capable of costly self-control, or by the experience of being out of control that might impair individual judgement (Loewenstein, 1996), or by overconfidence in one's future self-control and ability to deal with excessive alcohol intake (DellaVigna and Malmendier, 2006; Moore and Healy, 2008).

Speeding and Other Motor Vehicle Offenses — Insofar as roadside police (active) enforcement of the new law was weak, intoxicated drivers could have avoided speed camera (passive) detections by driving safely. To assess this response, we use annual data from the Home Office (England and Wales) and Recorded Crime in Scotland at the police force level. We have information on speeding offences, driving without wearing a seat belt, and driving while using a mobile phone.

The DD estimates in Appendix Table A.15 show a substantial decline of more than 50% in speeding offense rates. But after accounting for group specific trends, the effect switches sign and becomes economically negligible (an increase of 0.1%) and statistically insignificant. This null effect is confirmed by the synthetic control results presented in Appendix Figure A.31. Seat belt and mobile phone offense rates were also considerably reduced as a result of the 2014 reform, even after controlling for group specific trends, by about 40 and 50%, respectively. Neither of such impacts, however, survives when we rely on the synthetic control design.

With data covering all drivers, we cannot be certain these same estimates apply to those who drove while being above the limit. This null result, however, is about as clear an indication as one can find of a response that is inconsistent with the conjecture that the new law induced Scottish drivers to behave more vigilantly on the road.

Other Types of Crime — Recorded crimes for illegal drugs are directly comparable between Scotland and the rest of Britain. For other crimes — which include robbery, serious assault, and sexual offenses — the comparison is less straightforward, due to differences in recording practices. For these other crimes, we have to make some (mild) definitional assumptions, which are spelt out in the Online Appendix. Modifying definitions slightly does not change the results. The Scottish data are collected annually from 2010 to 2017 by Police Scotland at the local council level. The data for England and Wales are also collected annually over the same time period by ONS at the Community Safety Partnerships level which, in the majority of cases, coincide with local authority districts. For all four types of crime, our outcomes are reported offenses per 10,000 population.

[47] Moral licensing can occur in a variety of contexts, from charitable donations and racist attitudes to consumer behavior and job hiring. For instance, individuals who have spent time volunteering may find it acceptable to forget to report additional income when filling out their tax return. Or people who take multivitamin pills and dietary supplements may perceive they obtain significant health advantages and then engage in unhealthy activities, e.g., walk less or smoke more (Chiou, Yang, and Wan, 2011).

The time trends displayed in Appendix Figure A.32 reveal an increase in sexual crimes in both Scotland and England and Wales, whereas robbery and drug offences have receded, and slightly more so in England and Wales than in Scotland. Attempted murder and serious assault ramped up in England and Wales starting from 2012, while they remained stable in Scotland over the sample period. Table 9 presents the DD results, which always include local council fixed effects to account for the differences in the data gathering process that varies at the local authority district level. We find a detrimental impact of the lower DDL reform on drug related crime rates of about 19%, suggesting that alcohol and illicit drugs are substitutes (DiNardo and Lemieux, 2001; Conlin, Dickert-Conlin, and Pepper, 2005; Crost and Guerrero, 2012; Anderson, Hansen, and Rees, 2013; Dragone et al., 2019). Although this result may be interpreted as a negative externality of the reform, it could also give an indication of more active policing, something we could not find for drink driving. This relationship, however, becomes smaller and statistically insignificant once we account for group specific trends. Figure 13, which displays the gaps between Scotland and synthetic Scotland as opposed to the gaps between Scotland and the placebos in the rest of Britain, backs up the findings that the 2014 DDL reform did not affect drug related crimes. The results on robberies show a similar pattern.

Looking at the DD estimates in column (c) of Table 9, we find that the reform led to a 36% reduction in sexual offense rates and a four-fold increase in serious assault and attempted murder rates, this last effect providing support to the idea of moral licensing and present-focused preferences. Although both of these impacts are large, the estimates from the synthetic control approach in Figure 13 point to a null impact for both types of crime. Assigning greater weight to the latter, more compelling estimates, we conclude that there is little evidence of spillovers, negative or positive, on other forms of crime.

F. Responses from the Alcohol Industry

Alcohol Sales — As mentioned in Section 2, alcohol producers and organizations voiced concerns that the lower DDL would have hurt businesses (Wright, 2015). The analysis in part C above indicates that individual alcohol consumption was not affected by the reform. We now check if that result is upheld when we take the industry perspective.

For each of the three constituent countries of Great Britain, aggregate yearly data on off-trade, on-trade, and total sales (measured in terms of alcohol units sold per adult, aged 18 or more) are provided by the Monitoring and Evaluating Scotland's Alcohol Strategy and collected by Nielsen. Appendix Figure A.33 plots the joint trends for England and Wales separately from Scotland's between 2000 and 2017. On-trade alcohol sales per capita showed similar levels and declining trends everywhere, both before and after the 2014 reform. Off-trade sales have been traditionally higher in levels, and much more so in Scotland. For both England and Wales, where trends were virtually identical, off-trade sales first increased up to 2010 and then stabilized for the rest of the period.

Appendix Table A.16 presents the DD treatment effect estimates of the impact of the new DDL on sales. None of the estimates are statistically significant and the effects are small, ranging from –1% to 3%.

Alcohol Prices — Here we consider whether aggregate alcohol prices, measured in pounds per unit of alcohol, changed as a result of the reform. Drink drivers, as all drinkers and especially heavy episodic drinkers, may cut their habit in response to a permanent increase in alcohol prices (Becker and Murphy, 1988). On-trade alcohol prices were systematically higher, while off-trade prices systematically lower, in Scotland compared to England and Wales. This means the combined prices for the two types of alcohol sales were similar north and south of the Scottish border. Trends, and not just levels, were also similar and slightly increasing in both areas throughout the sample period (see Appendix Figure A.34). But, as shown by the difference-in-difference estimates in Appendix Table A.17, the 2014 Scottish BAC reform had no impact on alcohol unit prices.[48]

Labor Market — Another domain of the alcohol industry in which the reform could have had an impact is the labor market. To this purpose, we consider local authority administrative data on the number of pubs per 1,000 population and the number of pub jobs per 1,000 population.[49] The data are published annually by the Office for National Statistics and the Inter-Departmental Business Register over the period 2009–2016. The DD results in Appendix Table A.18 indicate that both the 5% increase in the number of pubs and the 9% cut in the number of pub jobs become smaller and statistically insignificant when we control for group specific trends and local authority fixed effects.

The spatial RD estimates in Appendix Table A.19 show modest, but significant, negative impacts of the stricter law on both the number of pubs and the number of pub jobs up to 100km from the borders. The estimates at lower distances however confirm the DD results that the reform had no effect on both labor market outcomes. Similarly, the gaps between Scotland and synthetic Scotland and the gaps between Scotland and the placebos in the whole of England and Wales shown in Appendix Figure A.35 lead us to reach the same zero-effect conclusion.

Other Responses — There might have been other responses — such as off-trade discounts, on-trade happy hours, and local advertising — for which we do not have reliable data. A recent qualitative study conducted with 16 owners and managers of on-trade premises in Scotland in 2018 finds that businesses responded to the reform by expanding the provision of no/low-alcohol alternatives (but no special offers on alcoholic drinks, neither before nor after the 2018 MUP reform), improving travel options (e.g., partnering with local taxi companies or offering transport deals), and training staff to

[48]We ought to remind that Scotland introduced a minimum unit pricing (MUP) of alcohol at 50 pence per unit on 1 May 2018. As argued in the Introduction, the MUP reform falls outside the period of interest of our analysis and should be the focus of another work.

[49]Over the last 20 years, two-thirds of jobs in the alcohol industry are estimated to be in pubs, bars, and clubs, while alcohol producers (breweries and distilleries) provide only about 5% of the total employment in the sector (Institute of Alcohol Studies, 2017).

deal with drink drive questions by customers (Sumpter et al., 2020). The same study reported also no impact on profits. From this and other similar anecdotal evidence (for example, the adoption of smaller beer glasses in pubs; see McLeod [2015]), there appears to have been only modest, ad hoc adaptations of the alcohol industry in Scotland. We cannot find any evidence whether the same inconspicuous responses occurred south of the border.

G. Responses from the Automobile Industry

Vehicle Registration — Another set of responses to the lower limit could have come from the automobile industry. Using the DfT data described in Section 3 on motor vehicle registrations by local authority, we analyze the impact of the new legislation on the number of vehicles registered each month per 1,000 population at the local authority district level over the 2009–2016 period. The DD estimates in Appendix Table A.20 as well as the synthetic control results in Appendix Figure A.36 provide strong evidence of no impact of the reform on automobile registration rates. According to the spatial RD estimates in Appendix Table A.21, instead, we find a small decline of 0.7% in registration rates when we consider local councils that are 100km to/from the border. But at the 200km and 50km bandwidths the estimates are statistically indistinguishable from zero.[50]

Petrol Prices — If petrol prices in Scotland went up as a result of the stricter BAC limit, then Scottish drinkers would have had a disincentive to drive ceteris paribus.[51] Although they did not drive less than their English and Welsh counterparts, Scots might have faced higher prices nonetheless, as the zero impact on own vehicle's utilization could mask a great deal of heterogeneity in price responsiveness of petrol demand (Blundell, Horowitz, and Parey, 2012).

Monthly data from January 2009 to December 2016 are published by the Automobile Association at the regional level.[52] Appendix Figure A.37 plots the trends for the prices (in pence per liter) of unleaded, super unleaded, and diesel over the sample period after averaging over the regions within England and Wales. Prices in the three countries track each other almost perfectly both before and after the 2014 reform. The DD treatment effect estimates in Appendix Table A.22 document unequivocally that the reform led to no differences in petrol prices between Scotland and the rest of Britain.

Automobile Insurance Premiums — Motor vehicle insurers may internalize higher traffic violation and accident risks for specific categories of drivers (based, for instance, on age or driving experience)

[50]As emphasized by the UK Office for National Statistics, motor vehicle prices cannot be reliably measured at the regional level, in line with Eurostat practice for the calculation of PPPs for motor vehicles. See <https://www.ons.gov.uk/economy/inflationandpriceindices/articles/relativeregionalconsumerpricelevelsuk/2016.> This means we cannot analyze the impact of the reform on automobile prices.

[51]Of the three components that make up the price of petrol, two cannot be modified locally, namely, fuel duty and value added tax. But fuel companies could locally modify the third component, which comprises the fuel's wholesale price, distribution costs, and profit margins.

[52]The data compilers aggregate up all the three price components (fuel duty, value added tax, and the last component that includes fuel's wholesale price, distribution costs, and profit margins) mentioned above. Besides Scotland, we have other eight regions: London and South West, South East, East Anglia, East Midlands, West Midlands, Yorkshire and Humberside, North West, and North Wales.

imposing higher premiums. Additional penalties may extend to undesirable driving behavior, such as drink driving or speeding, and include experience rating provisions, whereby the premium paid at any given time depends on past claims filed by insurees (Chiappori and Salanié, 2014). Sloan and Githens (1994) find that premium surcharges deter drink driving. We then use data on the British insurance premium index, which tracks the quarterly movement of car insurance prices for nine broad regions between April 2012 and January 2016. This index is an average of the five cheapest quotes from two sources, the online price comparison market and the direct and broker market.[53] We present evidence separating out the two premium sources. For both Scotland and the rest of Britain, the data from insurers show a declining trend, with a gap of about £300 (lower in Scotland) remaining stable over the sample period (Appendix Figure A.38). The same picture emerges from the web data, although the premium levels are lower and the decline is less steep than in the online market. Appendix Table A.23 shows the DD estimates. Irrespective of the specification and type of market, they reveal that the new BAC law had no effect on automobile insurance premiums.

In sum, the 2014 DDL reform induced a strong anti-drink drive sentiment among the Scottish public. But this did not lead to any economically relevant externality. Scots did not drive less or more. Their alcohol consumption, healthy diets, and smoking habits did not change. The reform had no spillovers on motor vehicle offenses that might have not been related necessarily to drinking, such as speeding or driving while using a mobile phone. It also did not generate displacement effects towards other types of criminal activities, including illegal drug usage, robberies, and sexual offenses. The alcohol industry remained unscathed, with no changes in production, prices, or employment. Similarly, the car industry faced no variation in automobile registration rates, petrol prices, and insurance premiums.

7. Supply of Offenses

Our final analysis focuses on the empirical relationship between BAC levels and the number of road traffic violations and accidents. If between the 0.05 and 0.08 BAC levels the relationship were steep and positive, we expect an intervention like the 2014 reform in Scotland to be effective in reducing road accidents and deaths. If instead the relationship were flat, the potential for the same intervention would be considerably more circumscribed.

This exercise could be regarded as a reduced form analysis of the aggregate supply of offenses formulated by Becker (1968) and Ehrlich (1973). The starting idea is that there is, at the individual level, a function relating the number of offenses, O_j, to the probability of conviction, p_j, the punishment if convicted, f_j, and other variables, u_j, such as the relative returns to drink driving with respect to other legal activities and the willingness to drive under the influence. If all individuals were identical, this function could be regarded as an aggregate supply function in a given time period, $O = O(p, f, u)$, and could be interpreted as the cumulative distribution of a density

[53] More details on these data are in the Online Appendix.

function showing differences across individuals with respect to the minimum expected net gain that is sufficient to induce them to become drink drivers.

Since we have no information on p or f in the data, we assume that both p and f are increasing linear functions of BAC levels. Substituting these two expressions into $O(\cdot)$ leads to our reduced form specification, which thus links the number of violations and collisions to BAC levels.

To obtain reliable estimates of this relationship, we have to tackle two data issues. First, individual level breath test data with exact levels of BAC are not available for Scotland. Second, for each recorded accident, we ideally need to have breath test information from at least another random driver in the exact location where the crash was observed, on the same day of the week and at the same time of the day, one week after the recorded accident to replicate external conditions which are as close as possible to those that occurred at the time of the recorded accident. This will give us information about the relative crash risk among drivers with different blood alcohol concentration levels.

As for the first issue, we use highly detailed individual-level digital breath test data collected between 2009 and 2014 by all police forces in England and Wales, the only data in Britain that report the precise reading, time, and location of each breath test administered. Breath tests were carried out because of a moving traffic violation, or another road code violation (e.g., illegal parking), a road traffic collision, or suspicion of alcohol. To address the second problem, we use the estimates of the relative risk of a crash provided in Compton et al. (2002), who collected breath test data on drivers involved in crashes of all severities in California and Florida and on two additional random drivers at the same location, day of the week, and time of the day a week after the crash, serving as control group. Compton and colleagues then estimated relative crash risk models as a function of BAC levels using logistic regression techniques.

For each level of BAC observed in our data, we thus compute the relative risk of offenses, multiplying the proportion of police administered breath tests by the relative risk of a crash estimated by Compton et al. (2012) and normalising the relative risk to 1 when no alcohol was consumed. The results of this exercise are presented in Figure 14, where we show one relationship in which all road traffic violations and collisions in the data are used and another in which we exclude the tests performed as a result of suspicion of alcohol. The two vertical lines are drawn in correspondence to the old and new DDLs (35 and 22μg, respectively, or equivalently 0.08 and 0.05 BAC). The inset in the figure zooms in on the interval between the two limits, where we expect to observe the impact of the reform.

Given the supply of offenses is very flat up to about 40 micrograms of alcohol per 100 milliliters of breath, a reduction from 35 to 22μg could not curtail the risk of motor vehicle accidents and road traffic violations substantially. The relative risk does decline, from 1.64 to 0.67 (or from 2.20 to 0.96, when excluding tests done on suspicion of alcohol), but the reduction is arguably trivial.[54]

[54]This is contrary to the information used by the Scottish government to increase awareness of the new DDL (see <https://www.wired-gov.net/wg/news.nsf/articles/Lower+drink+drive+limit+in+Scotland+041220

Put differently, the implied semi-elasticity of the risk of collisions to BAC is only 0.21 (s.e.=0.036) between the new and old limits, i.e., a 10% increase in BAC over this range augments the relative risk of collision by one-fifth of a point. This compares to substantially greater elasticities found at higher BAC values. For instance, the elasticity over the 0.08–0.10 BAC interval (which is relevant to most of the reforms in US states since the 1990s) is almost three time larger at 0.61 (s.e.=0.047), and the elasticity for BACs above 0.10 is a staggering 19.4 (s.e.=2.82), even higher than the estimates reported by Levitt and Porter (2001) and Romano et al. (2018) for fatal accidents only. Provided the patterns in the breath test data for England and Wales be good proxies for Scotland's and provided the relative crash risks estimated in the US be generalizable to Britain, the evidence in Figure 14 suggests the 2014 Scottish DDL reform did not have much ex-ante scope for a sizeable impact.

8. Conclusion

Summary — This paper evaluates the impact on motor vehicle crashes of a 2014 reform that reduced the drink drive limit from 0.08 to 0.05 BAC in Scotland, while in the rest of Britain the limit stayed at 0.08 BAC. Assembling several new data sources for the first time and using careful research designs, we conclude that the reform had no effect on accident rates, the main target of the Scottish lawmaker. This is the case for all types of accidents, from fatal crashes to collisions with only slight injuries, and regardless of whether drivers were drunk or sober. This null result holds for young and old drivers, men and women, whether crashes occurred during the day or at night, in weekends or workdays, and if they involved one or multiple vehicles. The result is also robust to several sensitivity checks, including manifold redefinitions of the outcome variable and finer categorizations of drink drive crashes, allowing for randomization inference, using count data models and linear panel event-study design, and combining matching and synthetic control approaches.

Establishing why the stricter BAC limit had no effect on the intended policy outcomes is a key contribution of our work. Using the insights of the canonical market model of crime, we focus on two mechanisms, namely alternative means of transportation and law enforcement. Taxis and buses were neither more available nor cheaper, as a result of the reform. Similarly, we find evidence of no impact on enforcement, measured broadly in terms of police numbers, breath tests carried out at the roadside, and drink drive arrests and convictions unrelated to motor vehicle crashes. Both channels, therefore, work jointly to explain why the new DDL law was ineffective in saving lives on Scottish roads.

Accompanied by a heavyweight media campaign, the reform ramped up anti-drink drive attitudes close to unanimity. This, however, was not followed by any positive externality. It was insufficient to induce Scots to drive less, curtail alcohol consumption, eat more fruit and vegetables, or improve

14102005?open>). Notice also that, when we consider all tests in Figure 14, the estimated relative risks around both thresholds (35 or 22μg) are below 1, which corresponds to the risk when no alcohol is consumed. A large number of offenses are observed even when drivers are legally sober or when they have no alcohol involvement at all.

smoking habits. There was no reduction in road offenses other than drink driving, such as speeding and driving while using a mobile phone or without a seat belt. The reform also did not reduce other crimes, including illegal drug use, robberies, and sexual offenses, and did not affect the alcohol and automobile industries, either negatively or positively. Finally, we find evidence that the reform could not have had much scope for a sizeable impact on road traffic accidents. The estimates from a reduced form version of the supply of offenses reveal only a modest collision elasticity to alcohol consumption over the critical 0.05–0.08 BAC range. This suggests that the pre-existing maximum legal level was already sufficiently low that further abatements in motor vehicle crashes could not have been easily reachable.

Are these results for Scotland generalizable to other countries? It is hard to come up with a definite answer (Pearl and Bareinboim, 2014). If one deems the Scottish environment — defined for instance in terms of pre-reform alcohol consumption levels and trends, road traffic accident rates, crash fatalities, and legal DDL — drastically different from all other environments, nothing can be transferred. If instead we have good reasons to believe it to be sufficiently similar, then one can justify the transportability of our findings to new targets. These may include countries like Ireland, Singapore, Mexico, Canada, and the US, where the patterns of either per capita alcohol intake, or accident rates, or both, are similar to Scotland's, and the drink drive limit is currently at 0.08 BAC.[55]

Final Remarks — Much ado about nothing, then? The stated expectation of the Scottish Parliament, Police Scotland, and the Scottish road safety authorities was that the stricter drink drive limit would have made roads safer and saved lives, by reducing the incidence of drink driving, pushing down conviction rates, lowering blood alcohol counts, and implementing a robust enforcement of the new law. Our results show that none of these margins changed as intended. Indeed, they did not change at all.

The reform, admittedly, was inexpensive. Apart from the resources needed for the advertising campaign, it was not supplemented with costly commitments, such as tighter police enforcement or greater public transport provision. But arguably this did not match the original stated objectives of the lawmaker. To give a chance to the tighter DDL law to be efficacious, as well as cost effective, its proponents might have pursued additional supporting interventions, account being taken of the small margins of success implied by the supply-of-offenses function estimates.

We stress two such public interventions, one for each of the two mechanisms that failed to sustain the reform. In line with the law and economics and crime prevention literatures (Chalfin and McCrary, 2017), one is to switch from broad but low-level patrolling to more focused hot-spot policing, with rotating checkpoints to mitigate the impact of driver learning and strategic responses (Banerjee et al, 2019). Another intervention bears on the taxi industry, with a combination of relaxing occupational licensing, liberalizing fares at times when social drinking is more concentrated,

[55]See <https://apps.who.int/gho/data/node.main.A997> and <https://apps.who.int/gho/data/node.main.A1036>.

and favoring the entry and operation of private ride-sharing companies, such as Uber and BlaBlaCar (Cramer and Krueger, 2016).

Naturally, the lawmaker could have introduced alternative public policies other than reducing the drink drive limit. One is the introduction of a minimum price per unit of alcohol. This indeed happened on the 1st of May 2018, outside our sample period, when Scotland set the minimum price at 50 pence per unit. In a pre-reform exercise, Griffith and Leicester (2010) argue that minimum unit pricing may imply large transfers to alcohol producers and off-license retailers and could penalize low-income households if these are more responsive to alcohol prices than richer households. Whether and how this new policy has prevented road deaths has not been studied yet.

Another option is to change consumer taxes on ethanol, varying tax rates across different alcohol types (e.g., beer, wine, spirits) and levels of alcohol strength. Griffith, O'Connell, and Smith (2019) show that optimally varying tax rates can lead to welfare gains which are larger when alcohol externalities are more convex in ethanol consumption. Knowing that the price elasticity of alcohol demand is non-trivial and tends to be higher in the long run (Cawley and Ruhm, 2012), many regard this option as promising (see for instance the review in Sloan [2020]). Recent evidence, however, shows that tax-induced hikes in alcohol prices can lead to substitution behavior which frustrates the attempted reductions in alcohol consumption (Gehrsitz, Saffer, and Grossman, 2020). Moreover, the UK has one of the highest average tax rates on alcohol among high income countries (Anderson, 2020), so further increases may be politically infeasible.

Lawmakers could rely on other instruments. One is to facilitate insurers to introduce higher experience-rated compulsory liability insurance premiums for drink drivers (Sloan and Githens 1994). Remaining in effect for years after a crash, premium increases could discourage drink driving, but could also lead to more uninsured motorists (Smith and Wright, 1992). Another measure is to increase the minimum legal drinking age above 18 years. Besides the likely social resistance to such a policy, our results by age do not suggest an extraordinary concentration of collisions among the very young. Yet another initiative is to rely more heavily on alcohol education in the attempt of improving, for instance, public awareness of the dangers of alcohol, the relationship between alcohol and impairment, safe drinking, and driving laws. But, as recognized by the *North Review*, it is unclear how much the public use this sort of information (North 2010, p. 74).

Driving under the influence may be deterred by tougher criminal punishments, such as longer imprisonments or driving bans. Lack of premeditation, however, could render such options too blunt and ineffective (Hansen and Waddell, 2018). An overlooked intervention is to redesign the pecuniary penalties associated with driving while intoxicated. Levitt and Porter (2001) and Francesconi and James (2019) estimate that drink driving generates large negative externalities, which are multiple times greater than the fines set out by law in the US and the UK, respectively. The last time fines were modified in the UK dates back to 1988, when the Road Traffic Act was enacted, except for sporadic inflation adjustments. The 2010 *North Review* expressed views on a number of existing and potential penalties for drink driving (including driving disqualification, vehicle confiscation, administrative license suspension and penalty points, and alcohol ignition interlocks), though almost nothing on fines (p. 82). This is surprising, with Becker (1968) showing that social welfare is increased if fines are used *whenever feasible*. The variable factors that are often involved in a drink drive case (such as pharmacokinetics, sanity, and premeditation) can enter into the determination of fines as proxies for the elasticities of offenses to changes in punishments and may enable courts to exercise discretion when sentencing offenders. There seems therefore to be no impediment to the feasibility of increasing fines.

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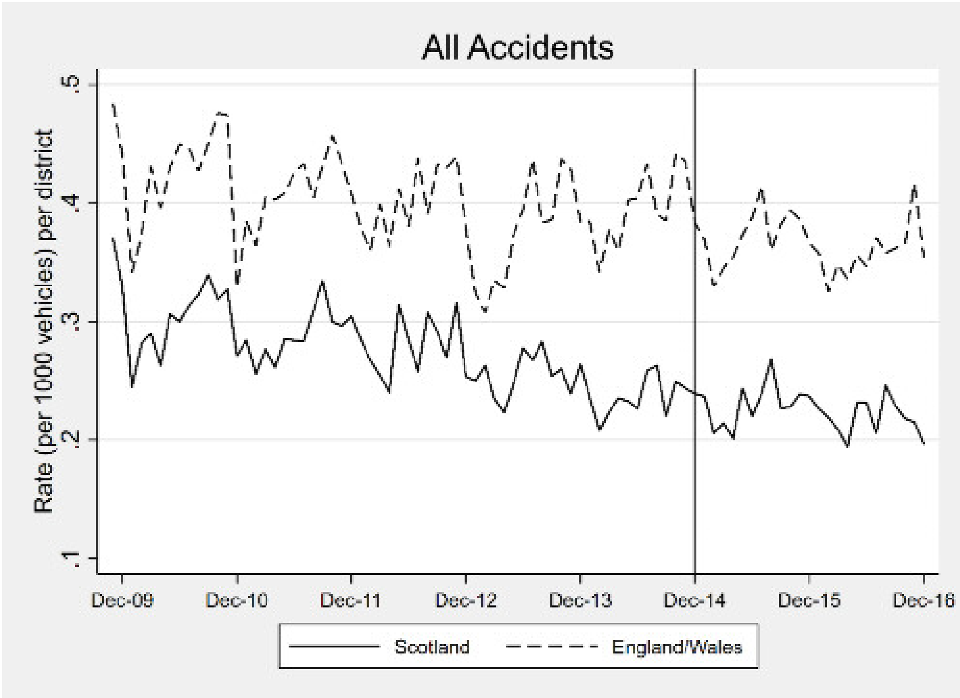
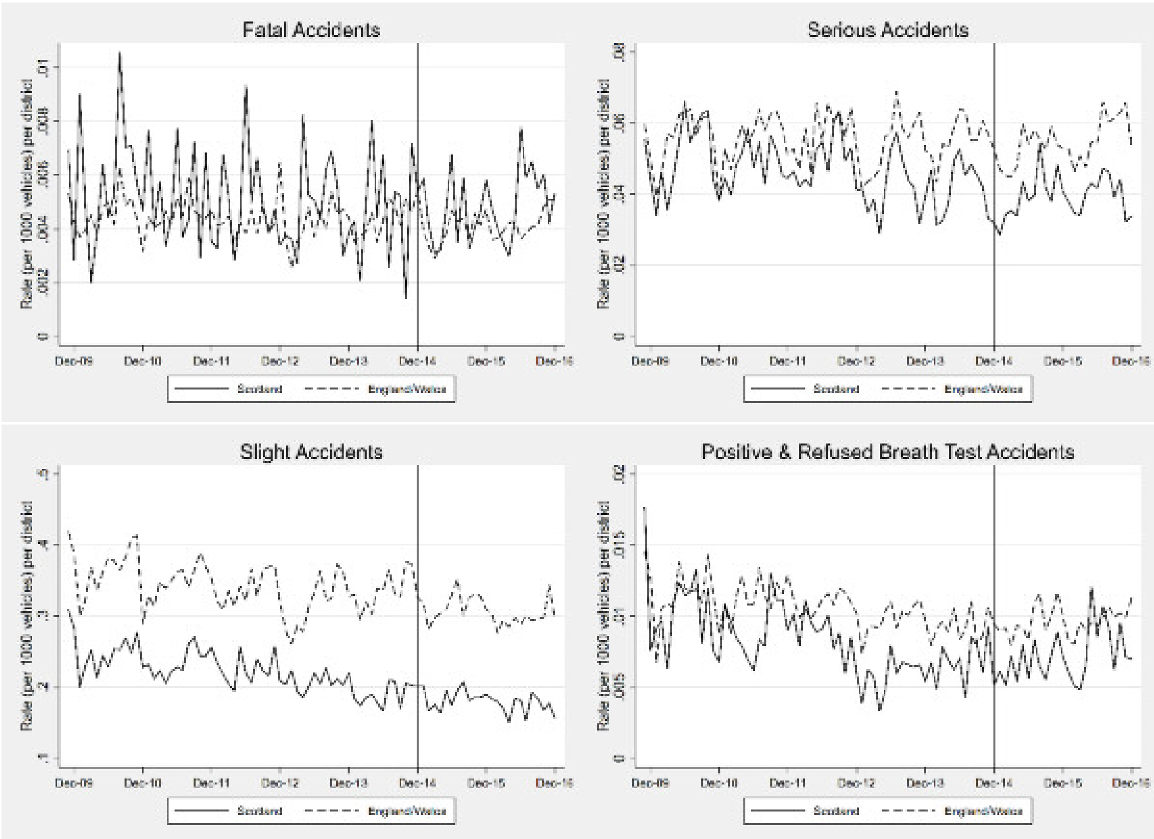
Figures and Tables

Figure 1: Trends in Road Accident Rates: Scotland versus the Rest of Britain



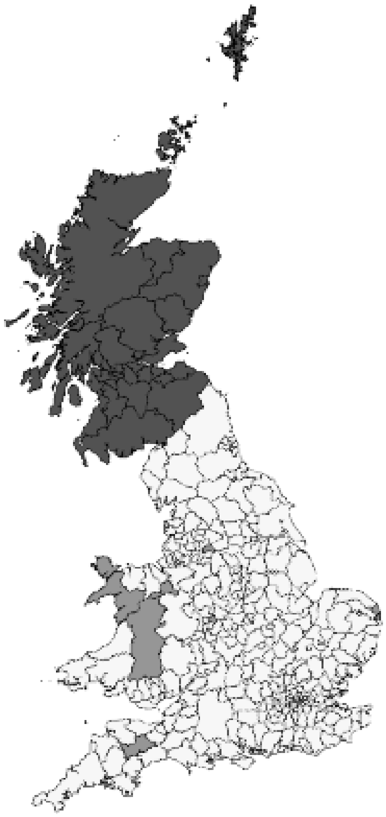
Sources: Road Accidents Data, Department for Transport, STATS19. Vehicle Licensing Statistics, Department for Transport.

Figure 2: Trends in Road Accident Rates, by Type: Scotland versus the Rest of Britain



Sources: Road Accidents Data, Department for Transport, STATS19. Vehicle Licensing Statistics, Department for Transport.

Figure 3: Map of Great Britain: Scotland versus Synthetic Scotland (All Accidents)



Notes: Local authority districts in dark grey identify Scotland. Local authorities in light grey make up synthetic Scotland. These are as follows (weight ω_c in parentheses): Oldham (0.049), Walsall (0.057), Great Yarmouth (0.056), Castle Point (0.083), Mid Devon (0.216), Isle of Anglesey (0.155), Gwynedd (0.097), Wrexham (0.168), Powys (0.12).

Figure 4: Trends in All Road Accident Rates: Scotland versus Synthetic Scotland

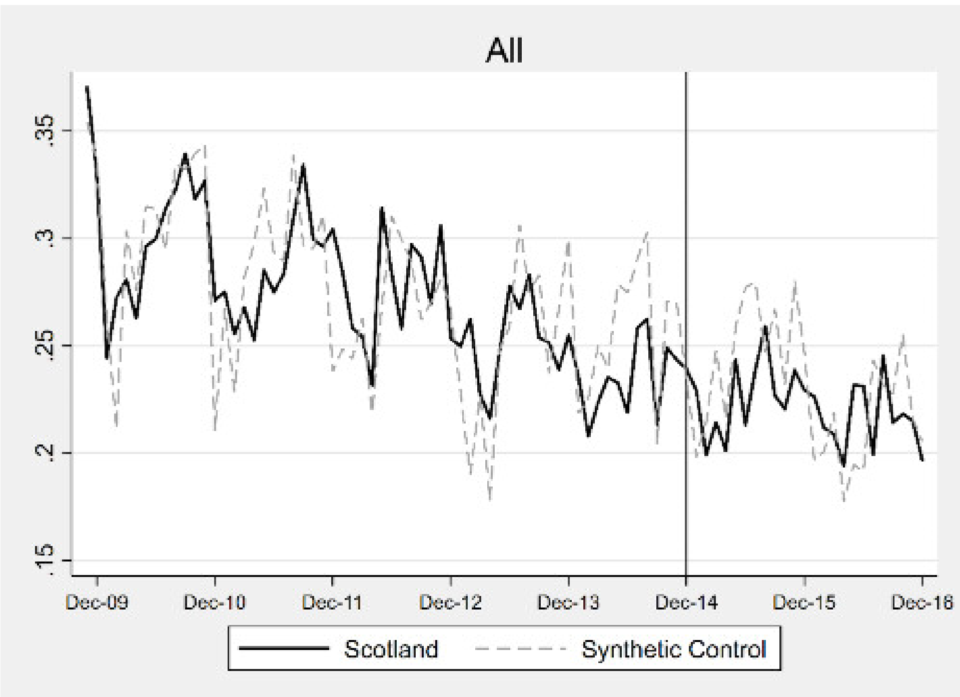


Figure 5: Trends in Road Accident Rates, by Type: Scotland versus Synthetic Scotland

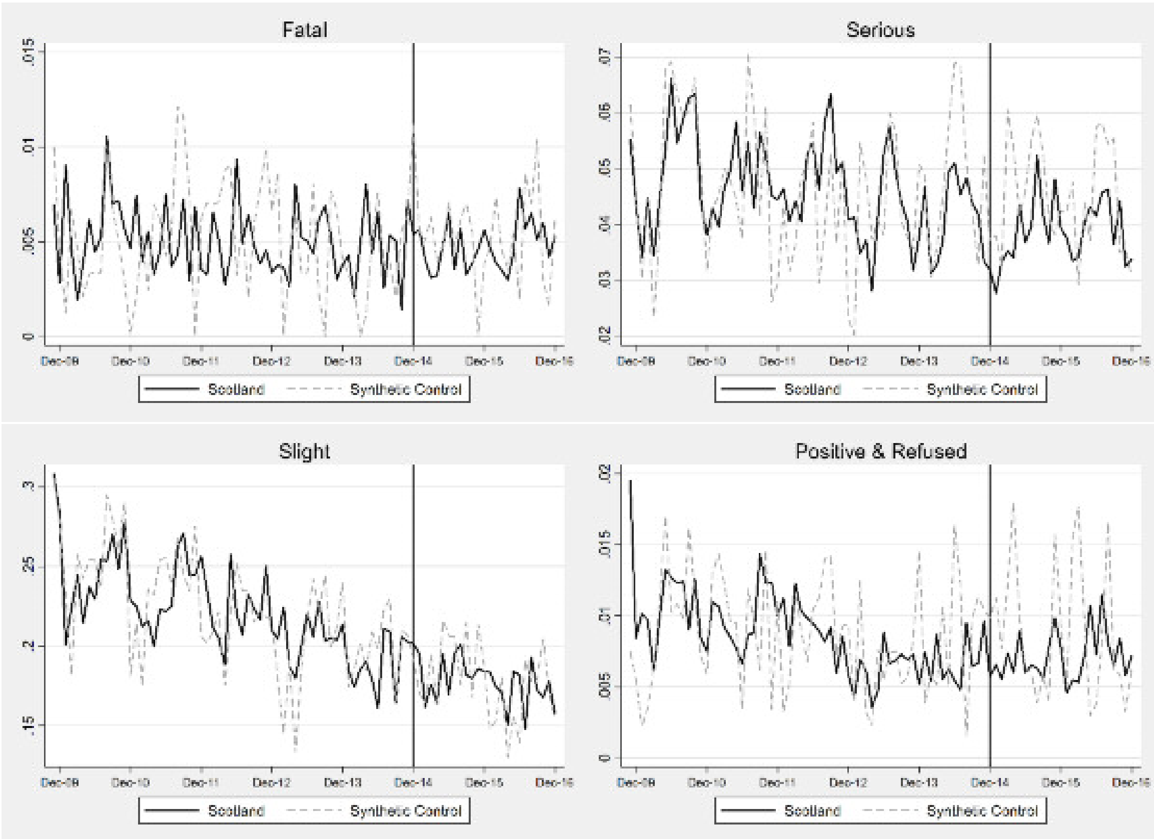
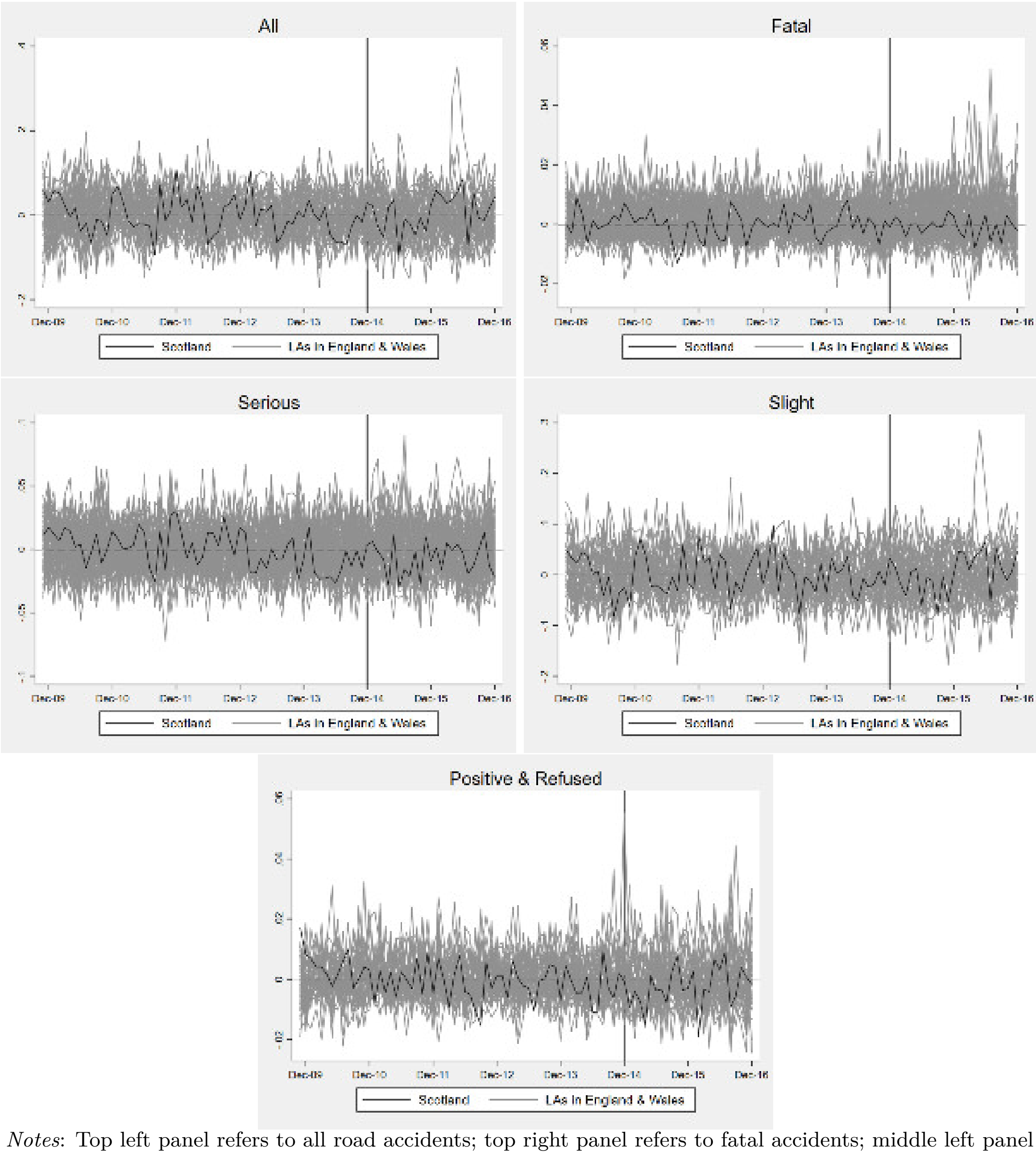
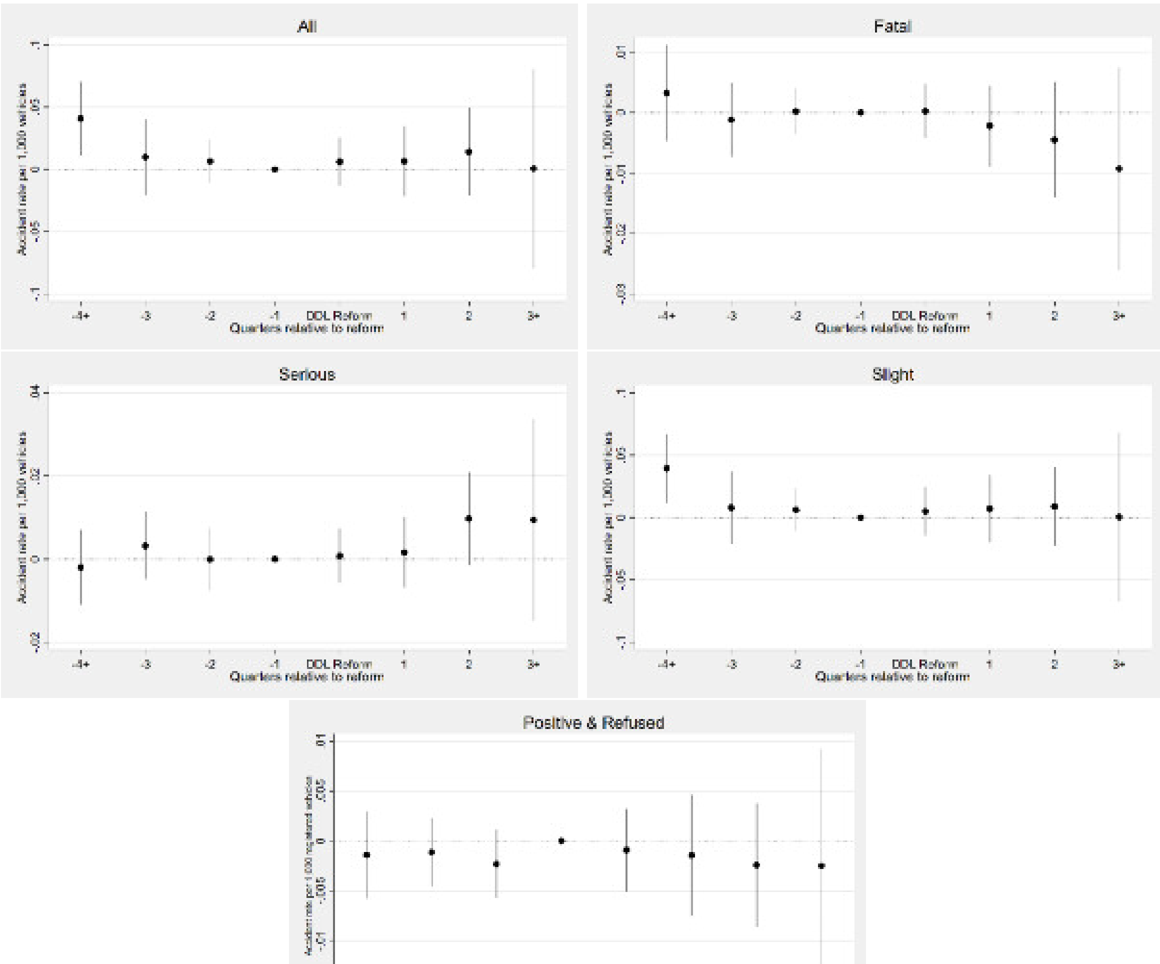


Figure 6: Gaps in Road Accident Rates for Scotland and Synthetic Scotland and for Scotland and Placebos in Control LAs, All Collisions and by Accident Type



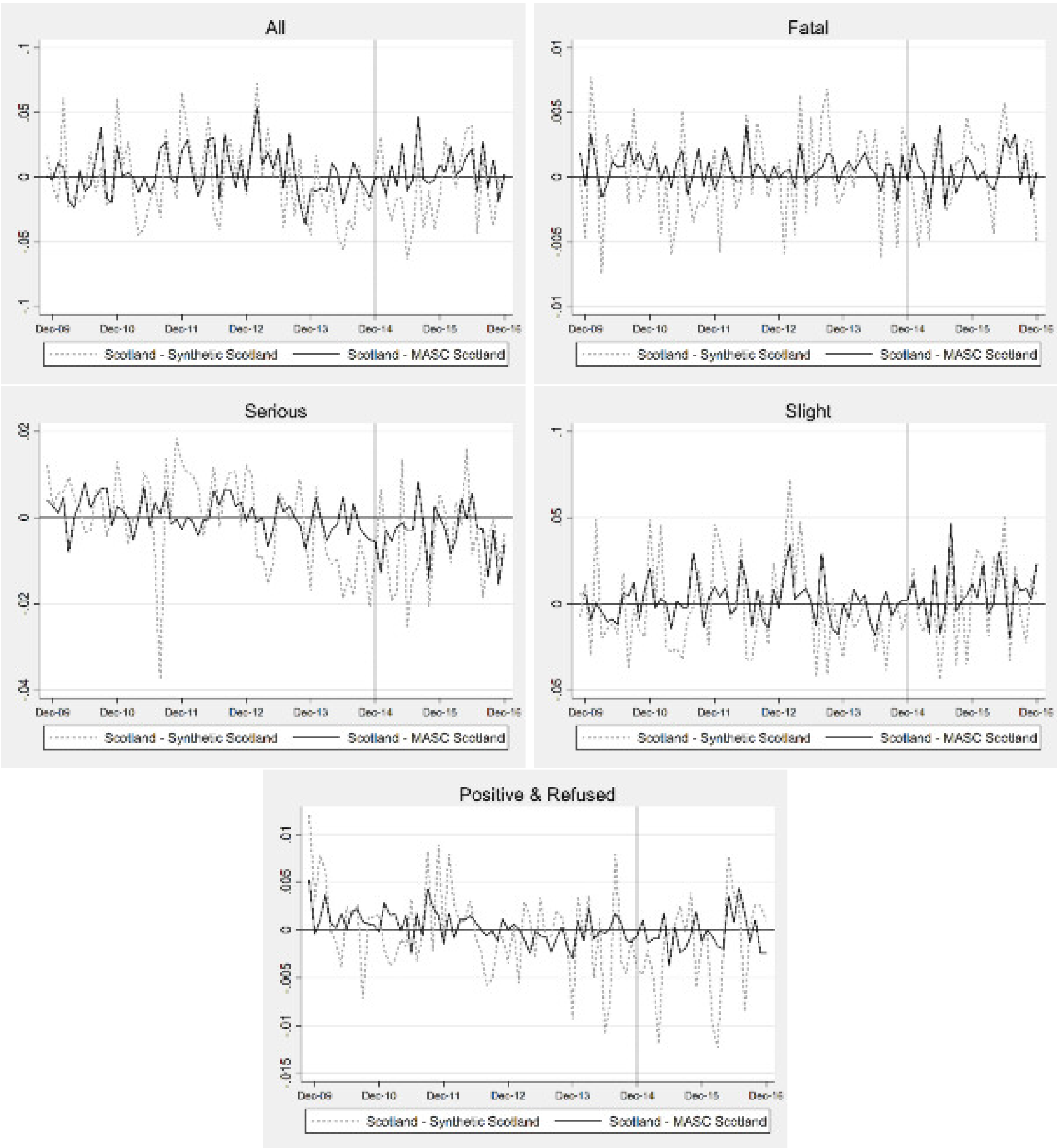
Notes: Top left panel refers to all road accidents; top right panel refers to fatal accidents; middle left panel refers to serious injury accidents; middle right panel refers to slight injury accidents; bottom panel refers to drink drive accidents (those with positive or refused breath test). In all panels, placebo districts with pre-reform mean squared prediction error (MSPE) that are two times higher than Scotland's are excluded. 'LAs' denotes local authorities.

Figure 7: Effect of the DDL Reform on Road Accident Rates at Quarters Around the Reform — 2SLS Estimates



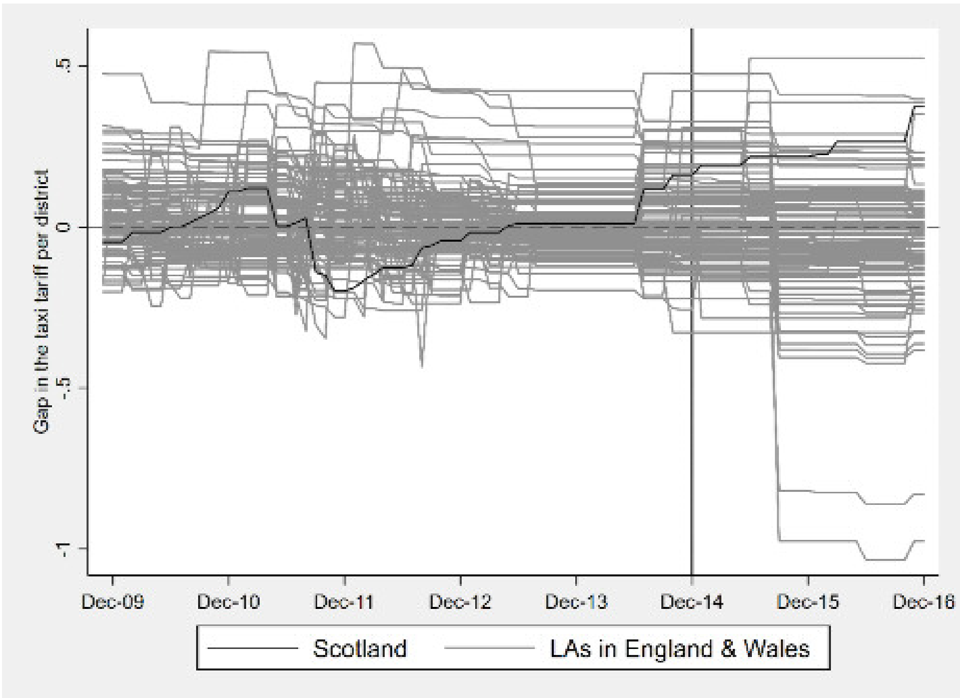
Notes: Each plot shows treatment estimates from a linear panel event-study version of (1), according to the procedure proposed by Freyaldenhoven, Hansen, and Shapiro (2019). One lead of the reform is used as excluded instrument for local unemployment rate (measured by the local Job Seekers' Allowance rate), our proxy for alcohol abuse (for a more detailed explanation, see the text). The vertical bars around each estimate are the 95% confidence intervals. Standard errors are clustered at the local authority level.

Figure 8: Gaps in Road Accident Rates for Scotland and Synthetic Scotland and for Scotland and MASC Scotland, All Collisions and by Type



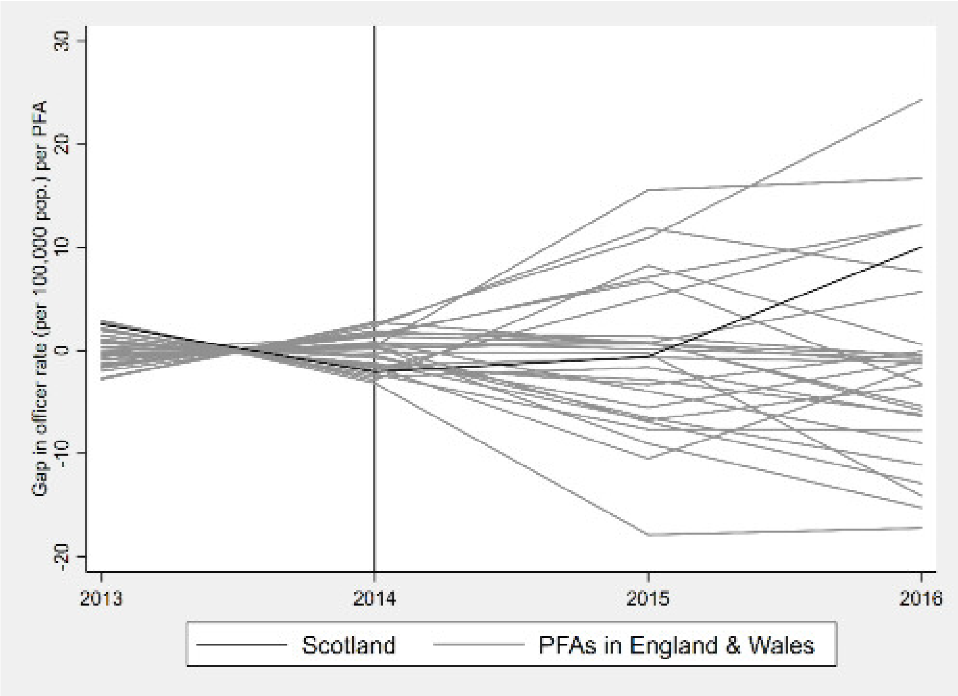
Notes: The gap (Scotland – Synthetic Scotland) shows the difference between the average road accident rate in Scotland and that produced by the synthetic control method of Abadie, Diamond, and Hainmueller (2010). The gap (Scotland – MASC Scotland) shows the difference between the average road accident rate in Scotland and that produced by the matching and synthetic control (MASC) approach proposed by Kellogg et al. (2019).

Figure 9: Gaps in Taxi Tariffs for Scotland and Synthetic Scotland and for Scotland and Placebos in Control LAs



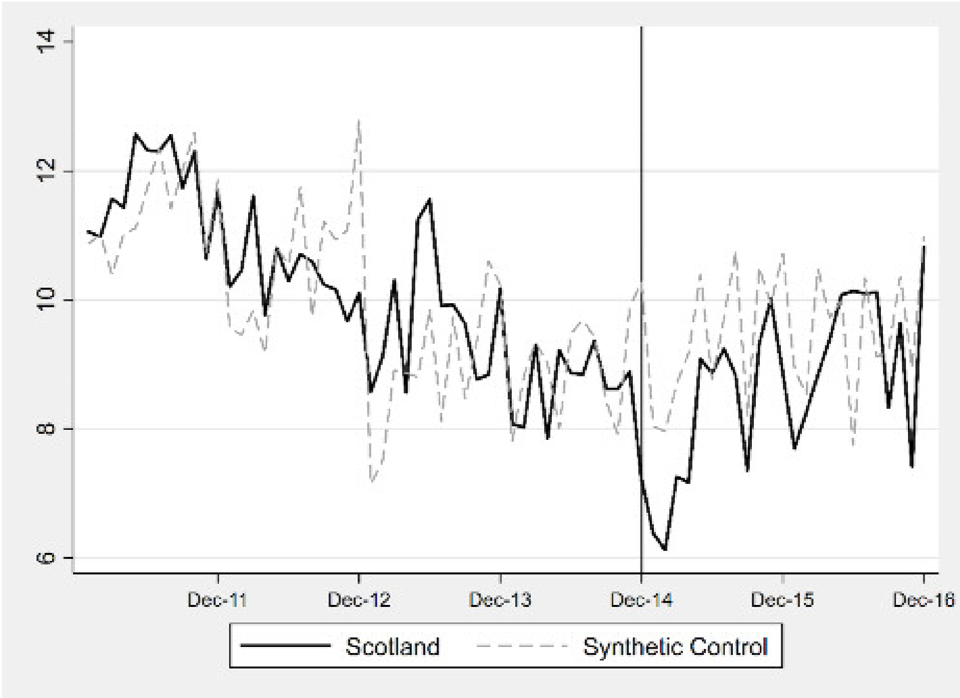
Notes: Placebo districts with pre-reform mean squared prediction error (MSPE) that are two times higher than Scotland's are excluded. 'LAs' denotes local authorities.

Figure 10: Gaps in Police Numbers for Scotland and Synthetic Scotland and for Scotland and Placebos in Control PFAs



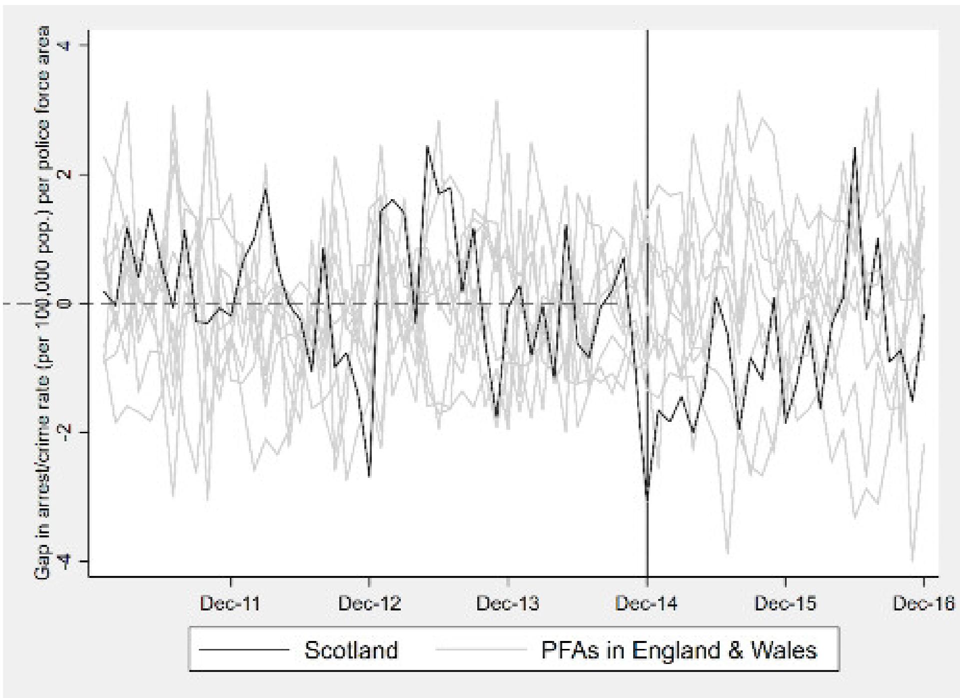
Notes: Placebo districts with pre-reform mean squared prediction error (MSPE) that are two times higher than Scotland's are excluded.

Figure 11: Trends in Drink Drive Arrest Rates: Scotland versus Synthetic Scotland



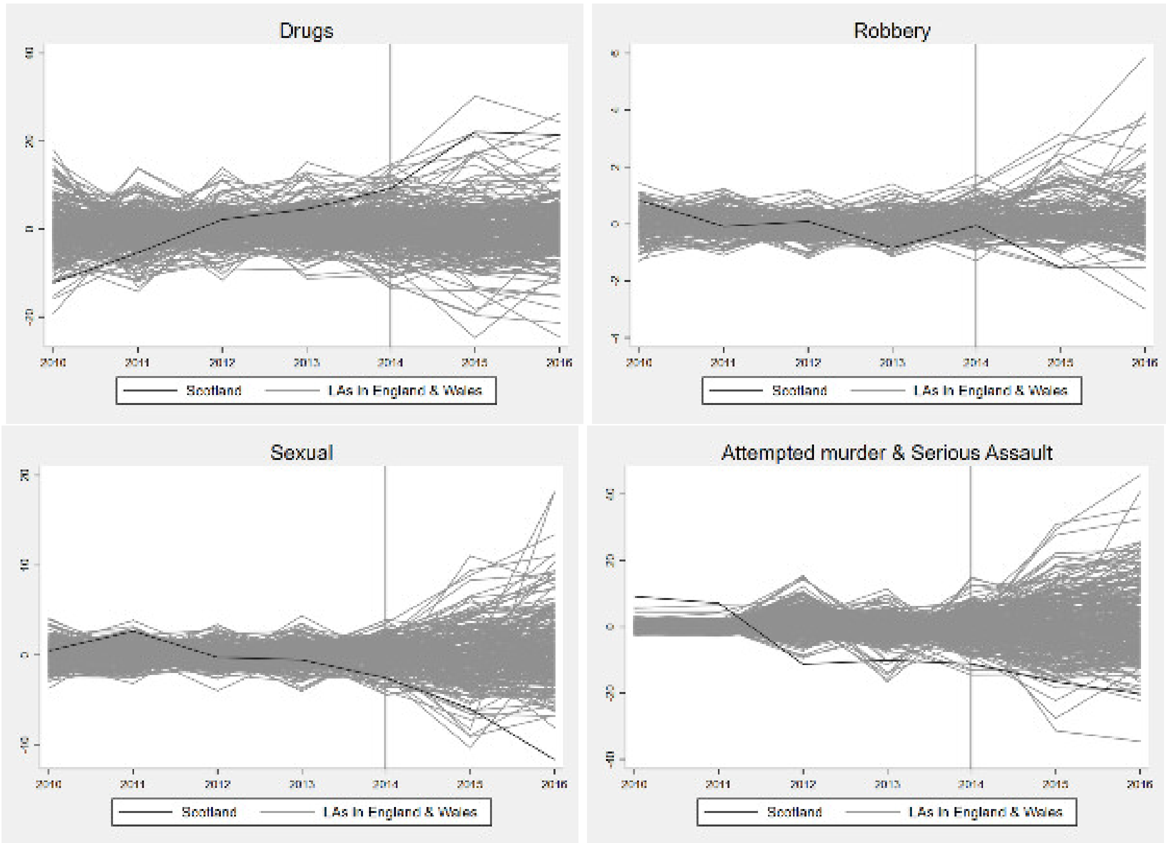
Notes: The sample period goes from January 2011 to December 2016.

Figure 12: Gaps in Drink Drive Arrest Rates for Scotland and Synthetic Scotland and for Scotland and Placebos in Control PFAs



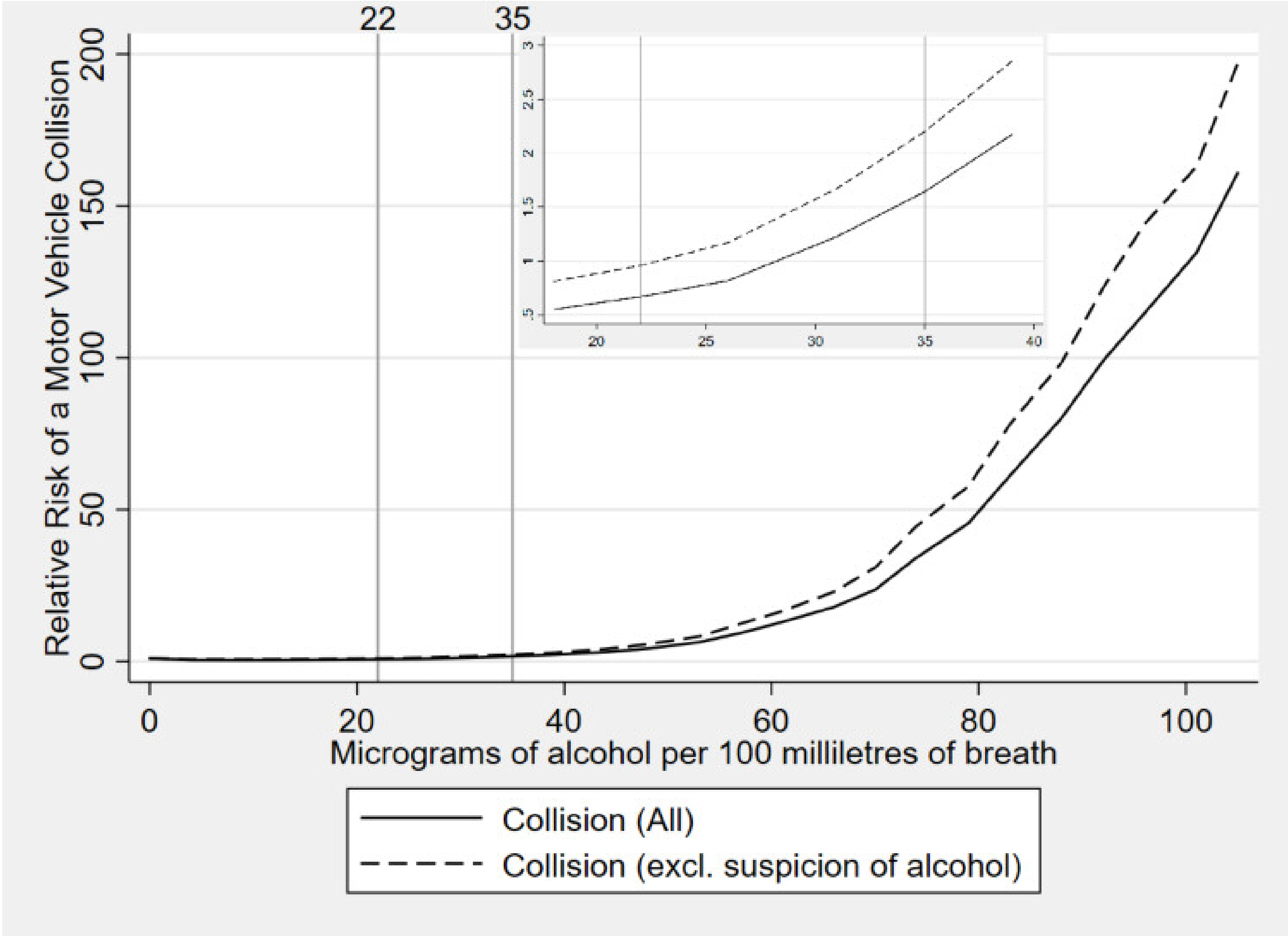
Notes: Placebo districts with pre-reform mean squared prediction error (MSPE) that are two times higher than Scotland's are excluded. 'PFAs' denotes police force areas.

Figure 13: Gaps in Other Crime Rates for Scotland and Synthetic Scotland and for Scotland and Placebos in Control LAs



Note: In all panels, placebo districts with pre-reform mean squared prediction error (MSPE) that are two times higher than Scotland's are excluded. Gaps are measured in crime rate per 10,000 population per district. 'LAs' denotes local authorities. The panels are: drug crimes (top left), robbery (top right), sexual offenses (bottom left) and attempted murder & serious assault (bottom right).

Figure 14: Adjusted Relative Risk of a Road Traffic Collision



Source: Road Safety Data, Digital Breath Tests, 2009–2014 (England and Wales).
Note: The breath test screening information comes from digital breath testing devices, as provided by police forces in England and Wales. The reasons for police-administered digital breath tests are: moving traffic violation; other road code violation (e.g., illegal parking); road traffic collision; suspicion of alcohol. The relative risk of a collision is calculated by multiplying the proportion of breath tests administered by the relative risk of a crash estimated by Compton et al. (2002), normalising the relative risk to 1 for cases in which no alcohol is consumed. The two vertical lines are drawn in correspondence to the old and new DDLs (35 and 22μg, respectively, or equivalently 0.08 and 0.05 BAC). The inset zooms in on the interval 17 and 38 BAC, which includes the two limits of interest.

Table 1: Pre-treatment Characteristics in Scotland, England and Wales, and Synthetic Scotland

	All LAs		LAs <100km		Synthetic Scotland				
	Scotland	England and Wales	Scotland	England and Wales					
Accident rates (per 1,000 vehicles)[a]									
All accidents	0.272	0.403	0.299	0.390	0.274				
	0.031		*0.000*						
Fatal accidents	0.005	0.004	0.004	0.006		0.005			
	0.595		*0.418*						
Serious injury accidents	0.046	0.056	0.047	0.052			0.046		
	0.328		*0.462*						
Slight injury accidents	0.221	0.343	0.247	0.332				0.223	
	0.021		*0.000*						
Positive/refused breath test accidents	0.009	0.011	0.008	0.012					0.009
	0.309		*0.067*						
Controls									
Temperature range (°C)[b]	6.828	7.688	6.971	7.207	7.186	7.412	6.940	7.007	7.123
	0.000		*0.340*						
Population density (pop./ha)[c]	4.038	12.69	4.326	7.108	4.022	4.035	4.576	4.039	4.635
	0.000		*0.006*						
Road length (km)[d]	1873	966.8	1836	1,617	1876	1685	1871	1872	1651
	0.000		*0.000*						
No qualifications (%)[e]	26.85	22.47	27.51	24.88	26.01	26.48	26.82	26.85	26.68
	0.000		*0.000*						
Very bad/bad health (%)[e]	0.053	0.054	0.054	0.066	0.059	0.054	0.065	0.067	0.060
	0.431		*0.000*						
Median working hours[f]	35.88	36.76	35.92	37.02	36.63	36.54	36.58	36.87	36.03
	0.000		*0.000*						
Job Seeker's Allowance (%)[f]	3.509	3.027	3.863	3.552	3.265	3.510	3.505	3.510	3.618
	0.009		*0.055*						
Nr. of licensed premises[g]	526.7	584.3	594.7	712.6	527.3	553.7	729.4	590.3	682.8
	0.282		*0.146*						

Sources: [a] Road Accident Statistics STATS19 Department for Transport; [b] Met Office; [c] Office for National Statistics; [d] Department for Transport; [e] 2011 Census; [f] NOMIS (www.nomisweb.co.uk/); [g] Department for Culture Media and Sport, the Home Office, and the Scottish Government.

Notes: Italicized numbers are p-values of the t-test of equality between groups in the relevant columns. 'Temperature range' is in degrees Celsius at the month-Met Office region level (9 regions). 'Population density' is defined as the population aged 17 or more divided by the area (in hectares) and is measured at the annual level by local authority (LA). 'Road length' is the total road length (in kilometres) measured annually at the LA level. 'No qualifications' is defined as the percentage of usual residents aged 16 or more with no qualifications measured at the 2011 Census. 'Very bad/bad health' is the percentage of all usual residents with bad or bad good health measured at the 2011 Census. 'Job Seeker's Allowance' is the percentage of the LA resident population aged 16–64 claiming Job Seeker's Allowance every month. 'Nr. of licensed premises' is the yearly number of premises registered in the LA with a legal license to sell alcohol.

Table 2: Effect of the DDL Reform on Road Accident Rates — Difference-in-Difference Estimates

	Mean	(a)	(b)	(c)	(d)	(e)
		A. All Accidents				
β	0.272	-0.0130*	-0.0113	0.0208**	0.0112	0.0015
		(0.0070)	(0.0096)	(0.010)	(0.0091)	(0.0068)
		B. Fatal Accidents				
β	0.005	-0.0001	-0.0001	0.0005	0.0006	0.0005
		(0.0004)	(0.0004)	(0.0008)	(0.0008)	(0.0008)
		C. Serious Injury Accidents				
β	0.046	-0.0061***	-0.0062***	0.0028	0.0028	0.0015
		(0.0017)	(0.0019)	(0.0033)	(0.0031)	(0.0023)
		D. Slight Injury Accidents				
β	0.221	-0.0069	-0.0051	0.0175**	0.0079	-0.0004
		(0.0061)	(0.0082)	(0.0073)	(0.0068)	(0.0071)
		E. Accidents with Positive/Refused Breath Test				
β	0.009	-0.0006	-0.0006	0.0013	0.0014	0.0014
		(0.0006)	(0.0006)	(0.0009)	(0.0009)	(0.0009)
Observations		32,508	32,508	32,508	32,508	32,508
Scottish LAs		31	31	31	31	31
English/Welsh LAs		347	347	347	347	347
All LAs		378	378	378	378	378
Controls		N	Y	Y	Y	Y
Month-year trend		N	N	Y	Y	Y
Month year trend $\times$ Scotland		N	N	Y	Y	Y
Month FEs		N	N	N	Y	Y
Month FEs $\times$ Scotland		N	N	N	Y	Y
LAs fixed effects		N	N	N	N	Y

Notes: Observations are at the LA-month-year level. The dependent variable is the number of accidents per 1,000 registered vehicles. The sample period goes from November 2009 to December 2016. Standard errors in parentheses are clustered at the LA level. 'Mean' refers to the Scottish pre-reform mean of the dependent variable. 'Controls' are LA monthly averages of temperature range, population density, proportion of residents aged 16 or more with no educational qualification, proportion of residents with bad or very bad health, median total hours worked, median gross pay, Job Seekers' Allowance rate, alcohol licensed premises, and total road length (see the text and the note to Table 1 for more details). 'LAs' denotes local authorities, 'FEs' denotes fixed effects.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Effect of the DDL Reform on Road Accident Rates — Spatial Regression Discontinuity Estimates

	(a) All	(b) Fatal	(c) Serious	(d) Slight	(e) Positive/Refused Breath Tests
<200 km	0.0053	0.0014**	0.0024	0.0016	0.0027**
	(0.0076)	(0.0007)	(0.0027)	(0.0067)	(0.0010)
	[0.508]	[0.055]	[0.380]	[0.831]	[0.016]
Mean	0.286	0.005	0.048	0.232	0.0087
<100km	0.0072	0.0010	-0.0023	0.0084	0.0017
	(0.0124)	(0.0013)	(0.0040)	(0.0112)	(0.0018)
	[0.572]	[0.433]	[0.597]	[0.459]	[0.373]
Mean	0.299	0.004	0.047	0.247	0.0084
<50km	-0.0015	0.0024	-0.0071	0.0032	0.0009
	(0.0290)	(0.0029)	(0.0063)	(0.0310)	(0.0053)
	[0.964]	[0.461]	[0.341]	[0.896]	[0.835]
Mean	0.284	0.006	0.055	0.224	0.008

Notes: Observations are at the LA-month-year level. The dependent variable is the number of accidents per 1,000 registered vehicles. The sample period goes from November 2009 to December 2016. 'Mean' refers to the Scottish pre-reform mean of the dependent variable. Standard errors in parentheses are clustered at the LA level. Due to the small number of LAs, wild bootstrapped p-values computed using Webb weights (Webb, 2014) and 5,000 replications are in square brackets. For completeness, however, these are shown also for large bandwidths. The numbers of Scottish LAs are 27, 12, and 3 in the first, second, and third row, respectively. The corresponding numbers for England/Wales are 69, 14, and 4, respectively. From the top to the bottom panel, the numbers of observations are 8,256, 2,236, and 602. Besides the set of controls reported in the notes to Table 2, distance from the Scottish/English border and distance from the border interacted with Scotland (with English distances taking negative values) are also included.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Effect of the DDL Reform on Taxi Licence Rates — Difference-in-Difference Estimates

	Mean	(a)	(b)	(c)	(d)	(e)
		A. Driver licences per 10,000 heads of population				
β	64.8	-5.893***	-5.474***	-6.010***	-1.659	-2.018
		(1.474)	(1.490)	(1.492)	(2.172)	(2.231)
		B. Vehicle licences per 10,000 heads of population				
β	35.7	-2.465***	-2.480***	-2.662***	0.414	0.154
		(0.818)	(0.889)	(0.842)	(0.952)	(0.933)
Observations		1,374	1,374	1,374	1,374	1,374
Controls		N	Y	Y	Y	Y
Biennial trend		N	N	N	Y	Y
Biennial trend × Scotland		N	N	N	Y	Y
LAs fixed effects		N	N	Y	N	Y

Sources: Department for Transport, Taxi Statistics – Table TAXI0104; Office for National Statistics.
Notes: Observations are at the LA-year level. The sample period goes from 2009 to 2015 (biennially). Standard errors in parentheses are clustered at the LA level. 'Mean' refers to the Scottish pre-reform mean of the dependent variable. 'Controls' are LA yearly averages of temperature range, population density, proportion of residents aged 16 or more with no educational qualification, proportion of residents with bad or very bad health, median total hours worked, median gross pay, Job Seekers' Allowance rate, alcohol licensed premises, and total road length (see the notes to Table 1 for more details). 'LAs' denotes local authorities.
*** $p < 0.01$.

Table 5: Effect of the DDL Reform on Taxi Tariffs — Difference-in-Difference and Spatial Regression Discontinuity Estimates

	Mean	(a)	(b)	(c)	(d)	(e)	(f) <200km	(g) <100km	(h) <50km
β	5.07	0.115**	0.131***	0.101*	0.090*	0.085*	0.084	0.084	0.324*
		(0.049)	(0.048)	(0.052)	(0.053)	(0.050)	(0.054)	(0.103)	(0.166)
							[0.152]	[0.439]	[0.081]
Observations		30,616	30,616	30,616	30,616	30,616	7,826	2,150	516
Controls		N	Y	Y	Y	Y	Y	Y	Y
Month-year trend		N	N	Y	Y	Y	Y	Y	Y
Month-year trend × Scotland		N	N	Y	Y	Y	Y	Y	Y
Month FEs		N	N	N	Y	Y	Y	Y	Y
Month FEs × Scotland		N	N	N	Y	Y	Y	Y	Y
LAs fixed effects		N	N	N	N	Y	Y	Y	Y

Source: Private Hire and Taxi Monthly available at: <https://www.phtm.co.uk/taxi-fares-league-tables>.

Notes: Observations are at the LA-month-year level. The sample period goes from November 2009 to December 2016. The dependent variable is the taxi tariff in pounds sterling. 'Mean' refers to the Scottish pre-reform mean of the dependent variable. Standard errors in parentheses are clustered at the LA level. Due to the small number of LAs in the spatial RD regressions, wild bootstrapped p-values computed using Webb weights (Webb, 2014) and 5,000 replications are in square brackets. For completeness, however, these are shown also for large bandwidths. 'Controls' are yearly averages of temperature range, population density, proportion of residents aged 16 or more with no educational qualification, proportion of residents with bad or very bad health, median total hours worked, and median gross pay, Job Seekers' Allowance rate, alcohol licensed premises, and total road length (see the notes to Table 1 for more details). 'LAs' denotes local authorities, 'FEs' denotes fixed effects.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: Effect of the DDL Reform on Drink Drive Arrest Rates — Difference-in-Difference Estimates

	Mean	(a)	(b)	(c)	(d)	(e)
β	12.0	-0.187	-0.147	-0.0756	-0.220	-0.158
		(0.382)	(0.389)	(0.372)	(0.364)	(0.345)
Observations		3,090	3,090	3,090	3,090	3,090
Month-year trend		N	Y	Y	Y	Y
Month-year trend $\times$ Scotland		N	Y	Y	Y	Y
Month FEs		N	N	Y	N	Y
Month FEs $\times$ Scotland		N	N	Y	N	Y
PFAs fixed effects		N	N	N	Y	Y

Notes: Observations are at the police force area-month-year level. The dependent variable is the number of drink drive arrests per 100,000 of the population. 'Mean' refers to the Scottish pre-reform mean of the dependent variable. The data are obtained from Freedom of Information requests to the 43 police forces in England and Wales and Police Force Scotland. Standard errors in parentheses are clustered at the Police Force Area (PFA) level. 'FEs' denotes fixed effects. The sample period goes from January 2010 until December 2016 for the following Scottish PFAs: Argyll and West Dunbartonshire, Ayrshire, Dumfries and Galloway, Edinburgh, Fife, Forth Valley, Greater Glasgow, Highland and Islands, Lanarkshire, North East, Renfrewshire and Inverclyde, Tayside, The Lothians and Scottish Borders, and for the following English PFAs: Cambridgeshire, Cheshire, Cumbria, Hampshire, Hertfordshire, Kent, Lancashire, Metropolitan Police, Norfolk, South Yorkshire, Staffordshire, Surrey, West Yorkshire. Other English and Welsh PFAs used in the analysis are for the following sample periods: North Wales: January 2012 to Decemer 2016; Dyfed–Powys: January 2011 to December 2016; Thames Valley: June 2012 to December 2016; West Midlands: August 2010 to December 2016; Bedfordshire: September 2016 to December 2016; Devon and Cornwall: April 2012 to December 2016; Dorset: May 2015 to December 2016; Cleveland: January 2010 to December 2012 and April 2013 to December 2016; Leicestershire: April 2010 to December 2016. For all other details, see the notes to Table 2.

Table 7: Effect of the DDL Reform on Attitudes toward Drink Driving — Difference-in-Difference Estimates

	Mean	(a)	(b)	(c)
		A. Should Not Drive If Drunk		
β	0.899	0.073***	0.085***	0.097**
		(0.027)	(0.027)	(0.042)
		B. DDL Knowledge		
β	0.755	0.006	0.004	0.034
		(0.042)	(0.042)	(0.061)
Observations		7,329	7,329	7,329
Controls		N	Y	Y
Linear annual trend		N	N	Y
Linear annual trend × Scotland		N	N	Y

Source: British Social Attitudes Surveys, 2009–2016.

Notes: Observations refer to the number of individuals in the sample. The dependent variables take value 1 if agreeing with the following statements: "If anyone has drunk any alcohol they should not drive?" (panel A, Should Not Drive If Drunk), "Most people don't know how much alcohol before being over legal limit?" (panel B, DDL Knowledge), and 0 otherwise. 'Mean' refers to the Scottish pre-reform mean of the dependent variable. Robust standard errors in parenthesis. In both panels, 'controls' are: age, sex, education (degree or more, higher education qualifications, A-levels (or equivalent), GCSE/O-levels (or equivalent), or foreign qualifications, with no qualification as the base category), ethnic origin (White, Black, or Asian, with others as the base category), and married/cohabiting. ** $p < 0.05$, *** $p < 0.01$.

Table 8: Effect of the DDL Reform on Alcohol Consumption — Difference-in-Difference Estimates

	(a)	(b)	(c)	(d)	(e)	(f)
	A. Units (on Heaviest Day)			B. Days Drank		
β	0.208**	0.208***	0.053	0.098***	0.070**	-0.024
	(0.081)	(0.077)	(0.112)	(0.033)	(0.032)	(0.045)
Mean		3.94			2.10	
Observations	128,898	128,898	128,898	118,176	118,176	118,176
	C. Units Usually Drunk per Week			D. 10+ Units		
β	0.498*	0.477*	0.530	0.006	0.007	-0.008
	(0.290)	(0.282)	(0.485)	(0.005)	(0.005)	(0.007)
Mean		10.80			0.128	
Observations	79,558	79,558	79,558	128,898	128,898	128,898
Controls	N	Y	Y	N	Y	Y
Linear annual trend	N	N	Y	N	N	Y
Linear annual trend $\times$ Scotland	N	N	Y	N	N	Y

Sources: Health Survey of England (England) and Scottish Health Surveys (Scotland), 2008–2016.
Notes: Observations correspond to the number of individuals over the sample period. The dependent variables are: the number of alcohol units drunk on heaviest day in the previous 7 days (top left); the number of days the interviewee drank over the past 7 days (top right); the number of alcohol units usually drunk per week, conditional on drinking, available in both England and Scotland from 2011 (bottom left); drinking 10 units of alcohol or more on the heaviest day (bottom right). 'Mean' refers to the Scottish pre-reform mean of the dependent variable. Robust standard errors are in parentheses. In all panels, 'controls' are: indicators of sex, marital status (married/cohabiting), ethnic minority (White, Black, or Asian, with others as the base category), education (leaving school at age 17 or after), and age (15 3-year age band groups).
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 9: Effect of the DDL Reform on Other Crimes and Offences — Difference-in-Difference Estimates

	Mean	(a)	(b)	(c)
		A. Drug Offences		
β	58.6	11.480***	11.323***	4.228
		(2.042)	(2.029)	(3.758)
		B. Robbery		
β	2.65	0.848**	0.778*	-0.530
		(0.356)	(0.418)	(0.414)
		C. Sexual Offences		
β	13.7	-4.089***	-3.885***	-4.951***
		(0.591)	(0.725)	(0.915)
		D. Attempted Murder and Serious Assault		
β	6.78	-38.052***	-32.871***	29.910***
		(1.108)	(3.090)	(1.353)
Observations		2,317	2,317	2,317
Controls		N	Y	Y
Linear annual trend		N	N	Y
Linear annual trend × Scotland		N	N	Y
CSPs fixed effects		Y	Y	Y

Sources: Recorded Crime in Scotland, 2010–2016 (Scotland); Recorded crime data at the Community Safety Partnership and Local Authority level (ONS), 2010–2016 (England).

Notes: Observations are at the local council (or CSP)-year level. There are 300 CSPs in England and Wales, and 31 in Scotland. For each type of crime, the dependent variables is the number of crimes/offences per 10,000 of population. (For definitions, see the Online Appendix.) 'Mean' refers to the Scottish pre-reform mean of the dependent variable. Standard errors in parentheses are clustered at the CSP level. 'Controls' are yearly averages of temperature range, population density, proportion of residents aged 16 or more with no educational qualification, proportion of residents with bad or very bad health, median total hours worked, median gross pay, Job Seekers' Allowance rate, alcohol licensed premises, and total road length (see the notes to Table 1 for more details). 'CPSs' denotes community safety partnerships (which correspond to local authority districts). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

ONLINE APPENDIX

Supplementary Material

Do Strict Drink Drive Limits Save Lives?

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University of Essex

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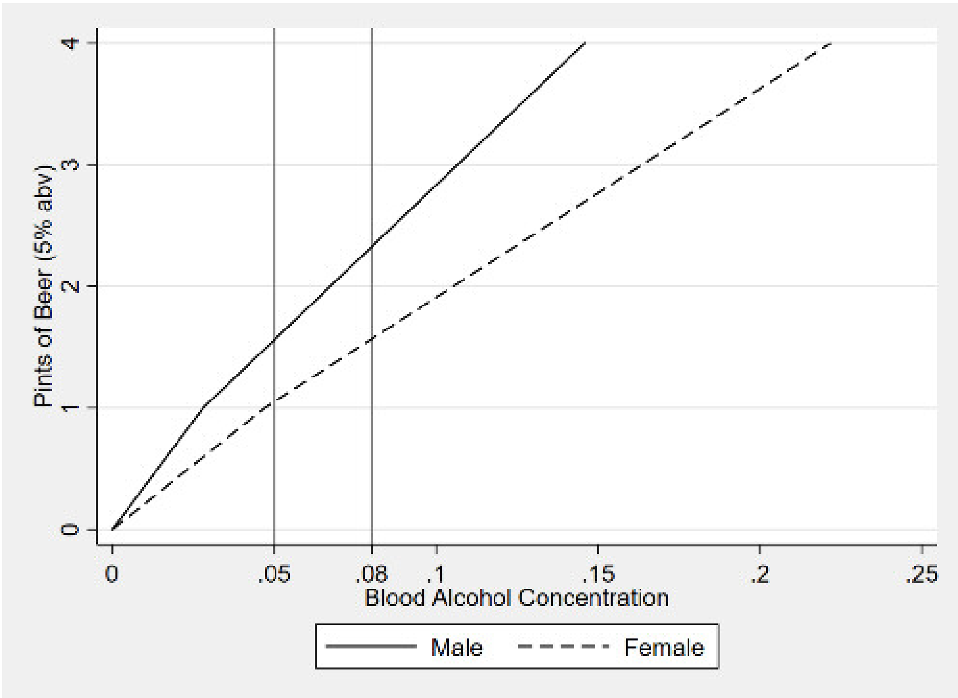
This Appendix reports additional analyses and results discussed in the main text, which could not be included due to space concerns. To locate the material more easily in the context of the paper, in what follows we use the same number and title of the sections used in the text.

2. Background

The Scottish Government's current anti-drink drive website can be found at: <https://roadsafety.scot/topics/drink-driving/>. The website used to be called Don't Risk It.[1] Additional information about the campaign was collated by the European Transport Safety Council (2016).

[1]For an archive copy, see <https://web.archive.org/web/20150501045404/http://dontriskit.info/drink-driving/the-law/>.

Figure A.1: Relationship between Alcohol Intake and Blood Alcohol Concentration

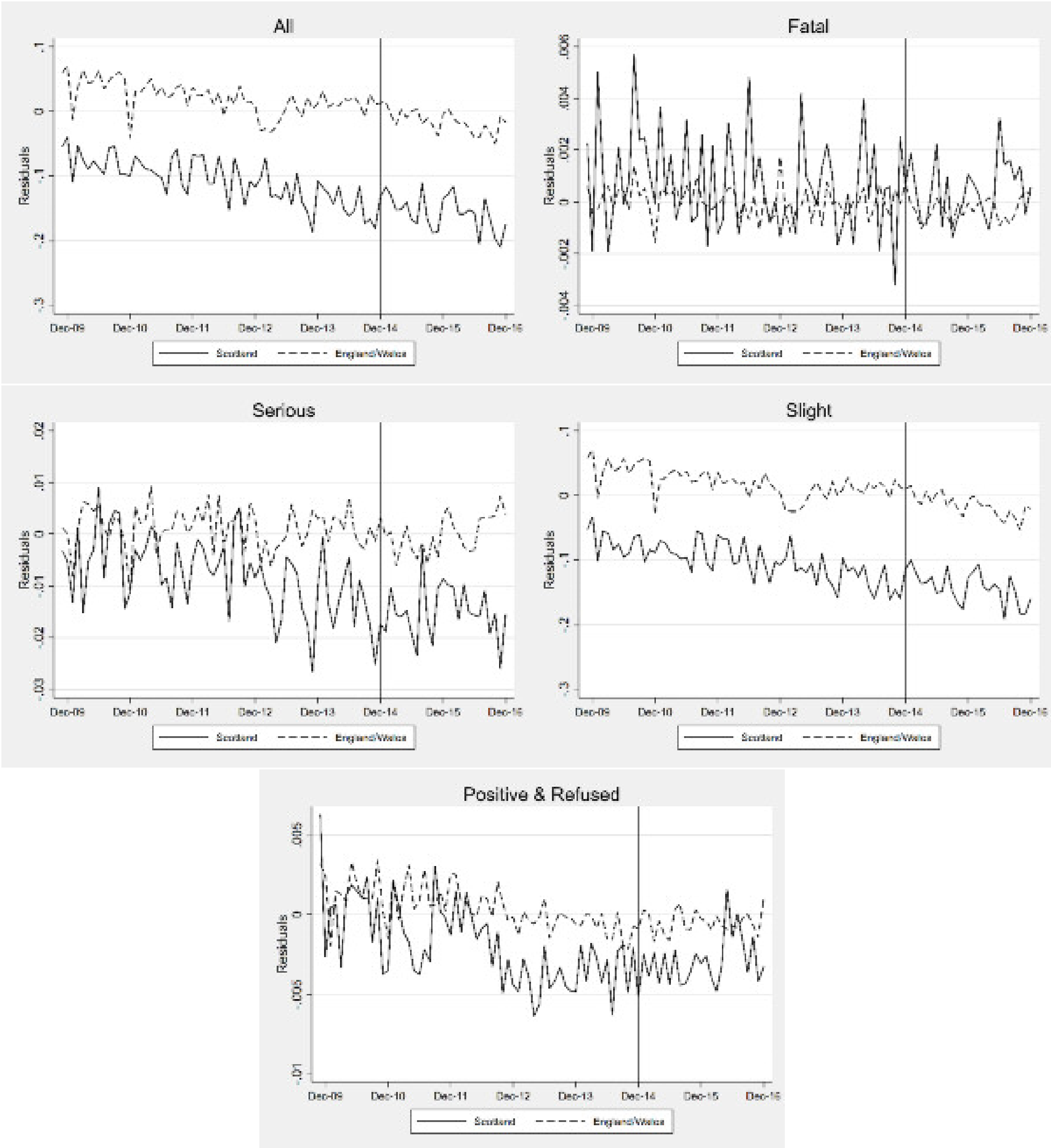


Source: `https://www.drinkdriving.org/drink_driving_information_bloodalcoholcontentcalculator.php`

Notes: Blood alcohol concentration (BAC) is expressed in grams of alcohol per deciliter of blood. Estimates are based on an average weight adult man (84kg) and woman (70kg) consuming alcoholic drinks over the space of one hour. Pints are defined as UK pints (or 568ml). The two vertical lines are drawn in correspondence to the old and new Scottish DDLs, i.e., 0.08 and 0.05 BAC, respectively.

3. Data and Methods Used for the Policy Evaluation

Figure A.2: Trends in Road Accident Rates Accounting for Seasonality: Scotland versus the Rest of Britain

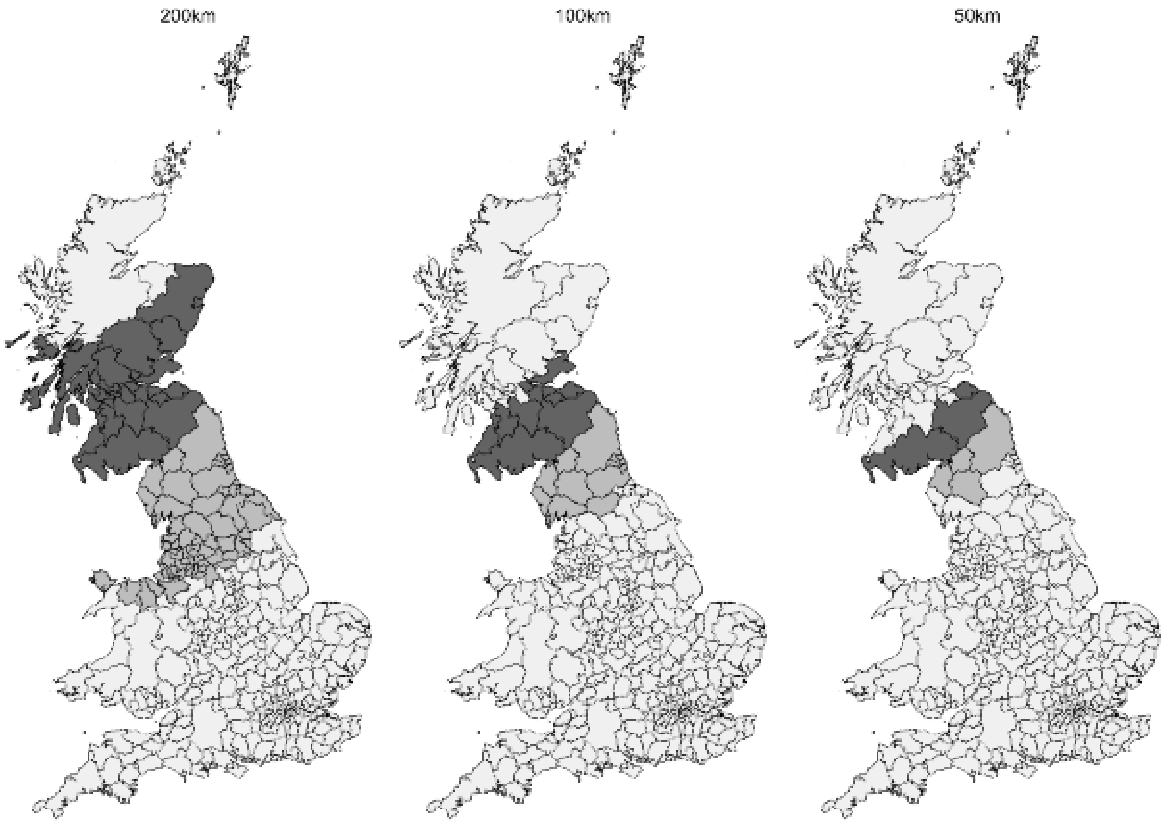


Sources: Road Accidents Data, Department for Transport, STATS19
Notes: Residuals are obtained from a regression of the road accident rate on a set of month of year dummies.

4. Results on the Policy Evaluation

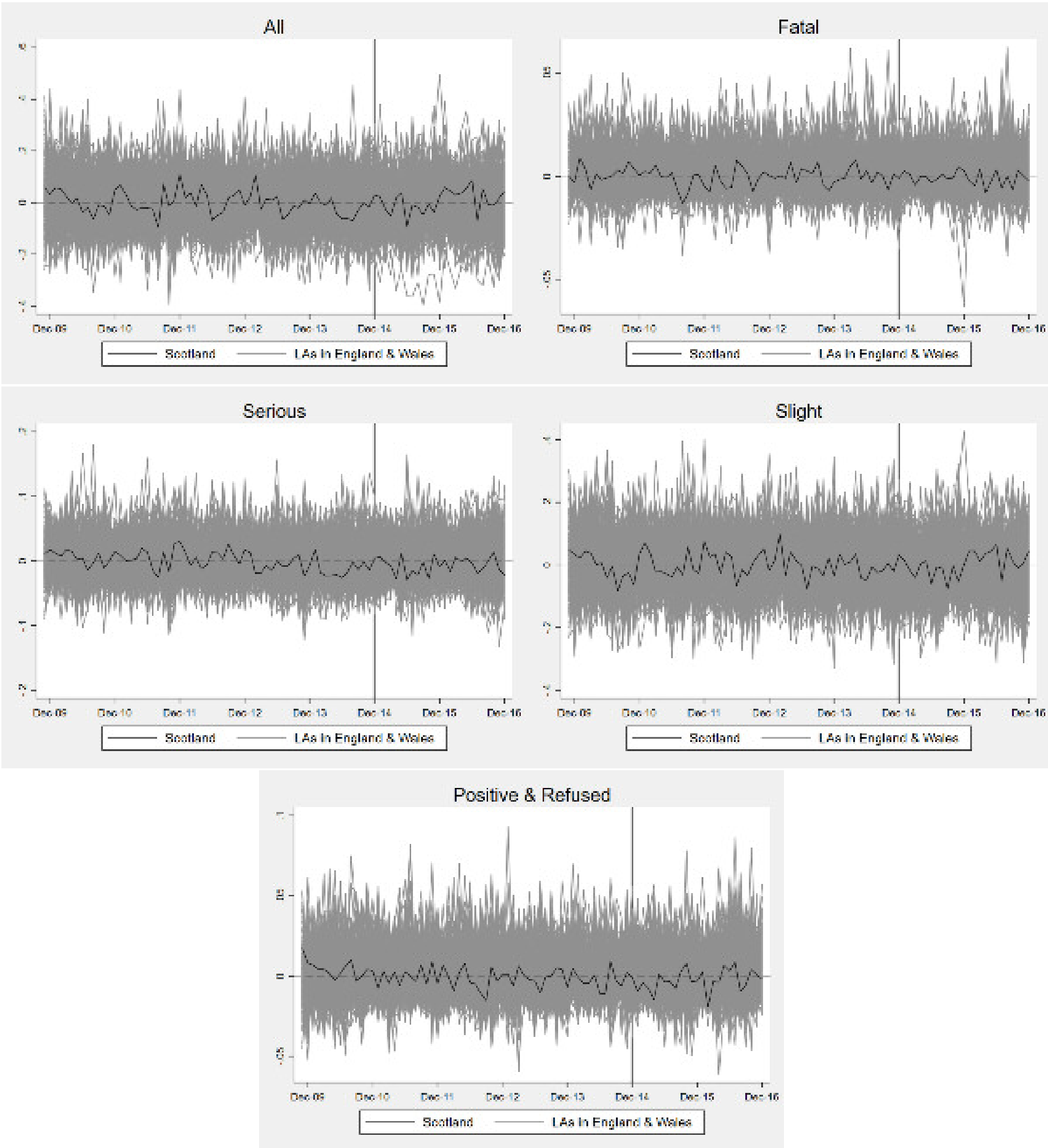
A. Benchmark Estimates

Figure A.3: Maps of Great Britain: LAs 200km, 100km, and 50km to/from the Scottish-English Border



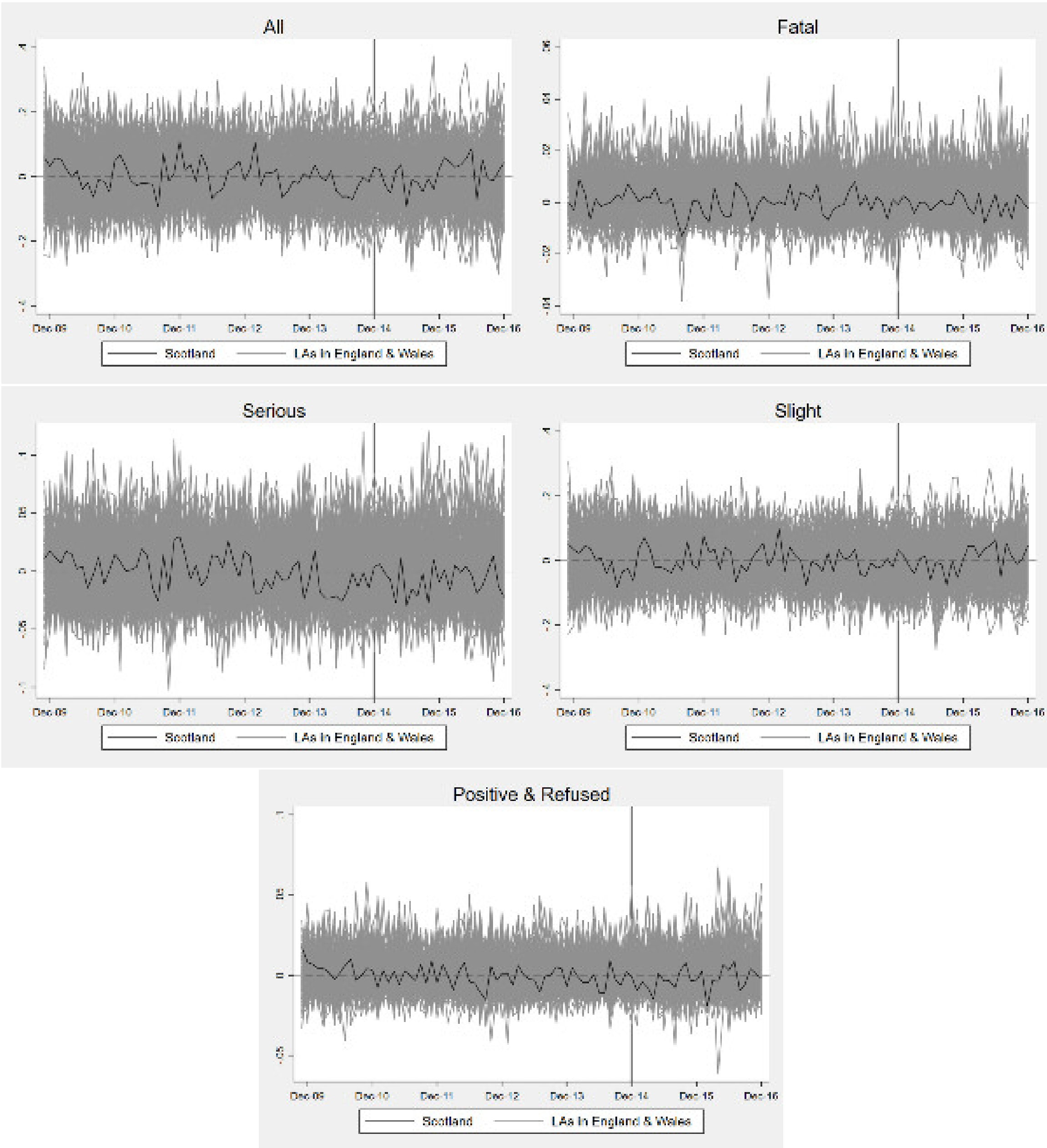
Notes: Local authorities (LAs) with their centroid (LA centre point) within 200km (left map), 100km (middle map) and 50km (right map) of the Scottish-English border.

Figure A.4: Gaps in Road Accident Rates for Scotland and Synthetic Scotland and for Scotland and Placebos in Control LAs, 10 MSPE



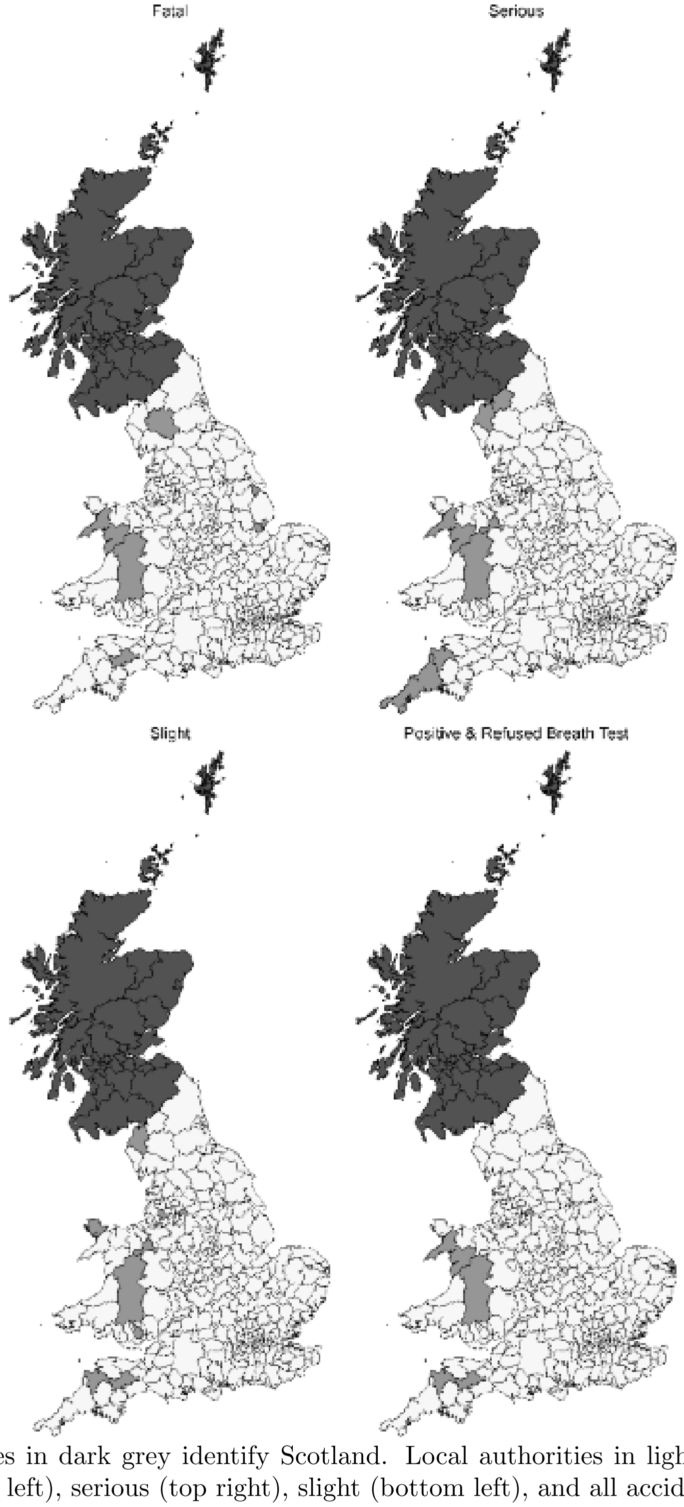
Notes: Top left panel refers to all road accidents; top right panel refers to fatal accidents; middle left panel refers to serious injury accidents; middle right panel refers to slight injury accidents; bottom panel refers to drink drive accidents (those with positive/refused breath test). In all panels, placebo districts with pre-reform mean squared prediction error (MSPE) that are 10 times higher than Scotland's are excluded.

Figure A.5: Gaps in Road Accident Rates for Scotland and Synthetic Scotland and for Scotland and Placebos in Control LAs, 5 MSPE



Notes: Top left panel refers to all road accidents; top right panel refers to fatal accidents; middle left panel refers to serious injury accidents; middle right panel refers to slight injury accidents; bottom panel refers to drink drive accidents (those with positive/refused breath test). In all panels, placebo districts with pre-reform mean squared prediction error (MSPE) that are five times higher than Scotland's are excluded.

Figure A.6: Maps of Great Britain: Scotland versus Synthetic Scotland, by Accident Type



Note: Local authorities in dark grey identify Scotland. Local authorities in light grey make up synthetic Scotland for fatal (top left), serious (top right), slight (bottom left), and all accidents with positive/refused breath test (bottom right). These are as follows (weight ω_c in parentheses):
Fatal: Eden (0.083), North East Lincolnshire (0.258), Boston (0.067), Castle Point (0.042), Thurrock (0.184), Mid Devon (0.092), Gwynedd (0.155), Powys (0.12).
Serious: Allerdale (0.07), Carlisle (0.218), Knowsley (0.061), Oldham (0.259), Torridge (0.099), Cornwall (0.063), Gwynedd (0.06), Wrexham (0.109), Powys (0.06).
Slight: Allerdale (0.096), Oldham (0.106), Wigan (0.105), Mid Devon (0.082), Torridge (0.056), Isle of Anglesey (.088), Wrexham (0.167), Caerphilly (0.142), Powys (0.158)
Positive/Refused Breath Tests: Oldham (0.292), Great Yarmouth (0.149), Castle Point (0.01), Mid Devon (0.142), Torridge (0.091), Gwynedd (0.309), Powys (0.006).

B. Heterogeneity

Figure A.7: Gaps in Road Accident Rates for Scotland and Synthetic Scotland and for Scotland and Placebos in Control LAs, by Time of the Day

(a) 8am - 8pm

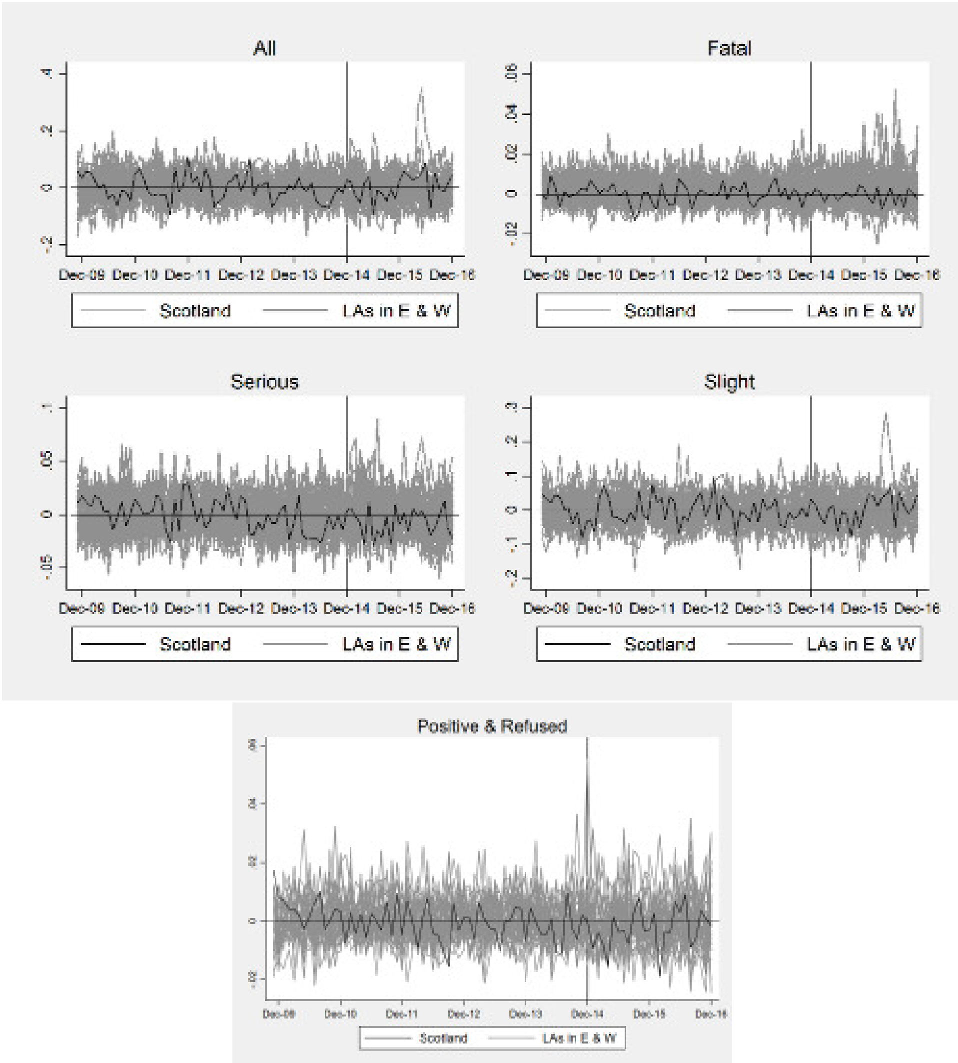
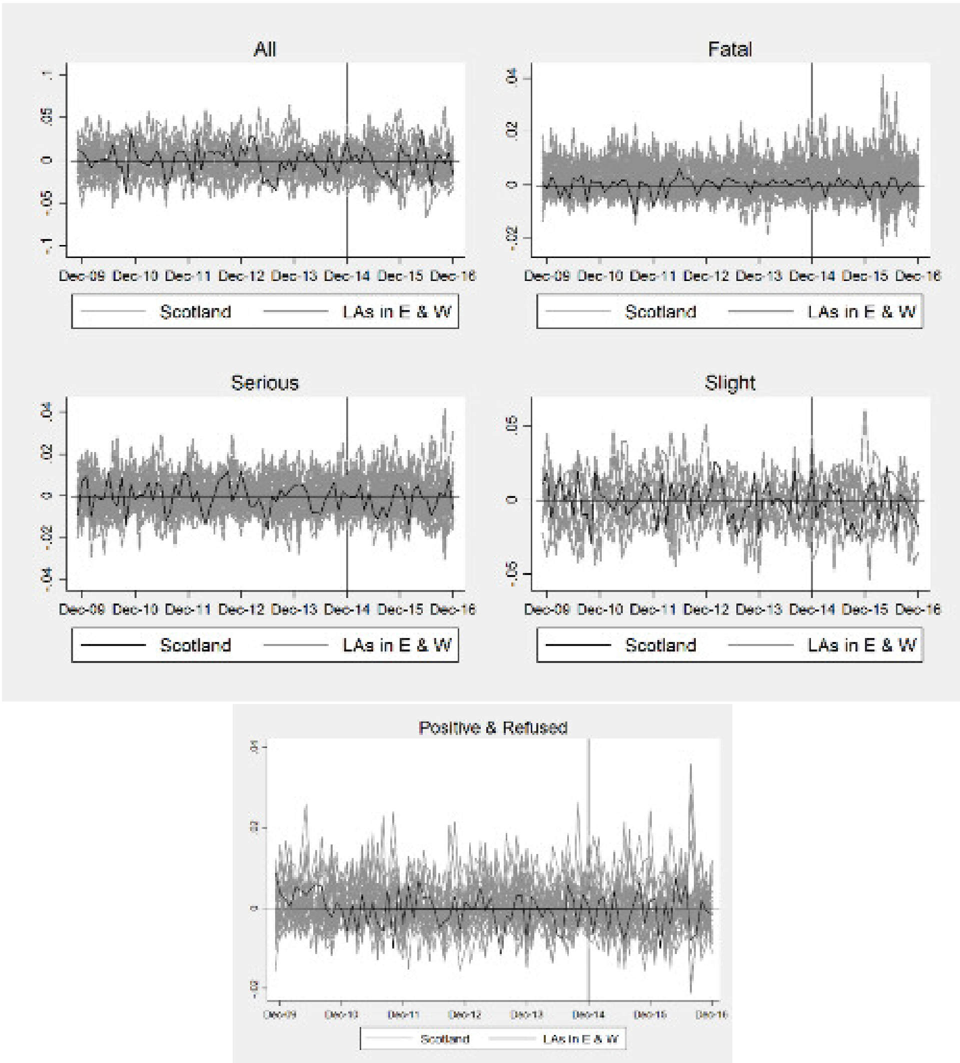


Figure A.7: Gaps in Road Accident Rates for Scotland and Synthetic Scotland and for Scotland and Placebos in Control LAs, by Time of the Day (cont.)

(b) 8pm - 8am



Notes: In all panels, placebo districts with pre-reform mean squared prediction error (MSPE) that are two times higher than Scotland's are excluded.

Figure A.8: Gaps in Road Accident Rates for Scotland and Synthetic Scotland and for Scotland and Placebos in Control LAs, by Day of the Week

(a) Saturday and Sunday

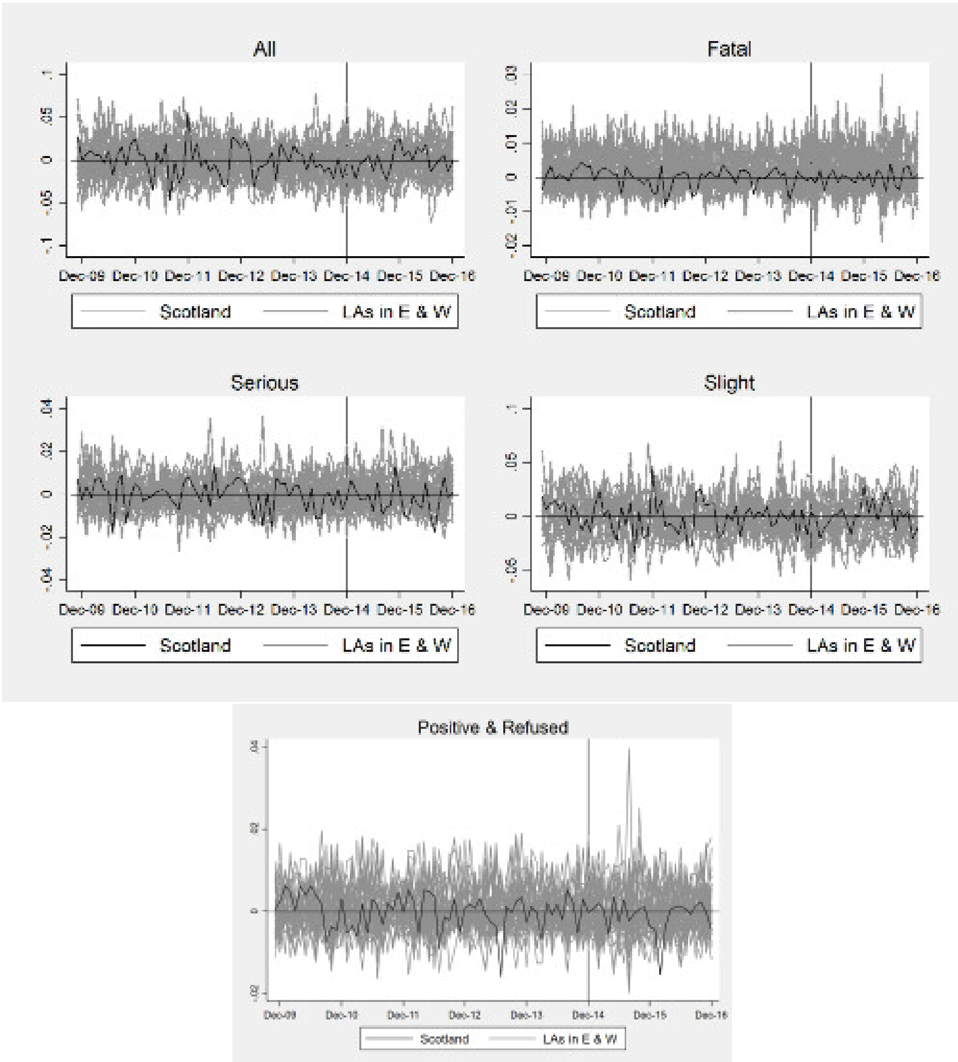
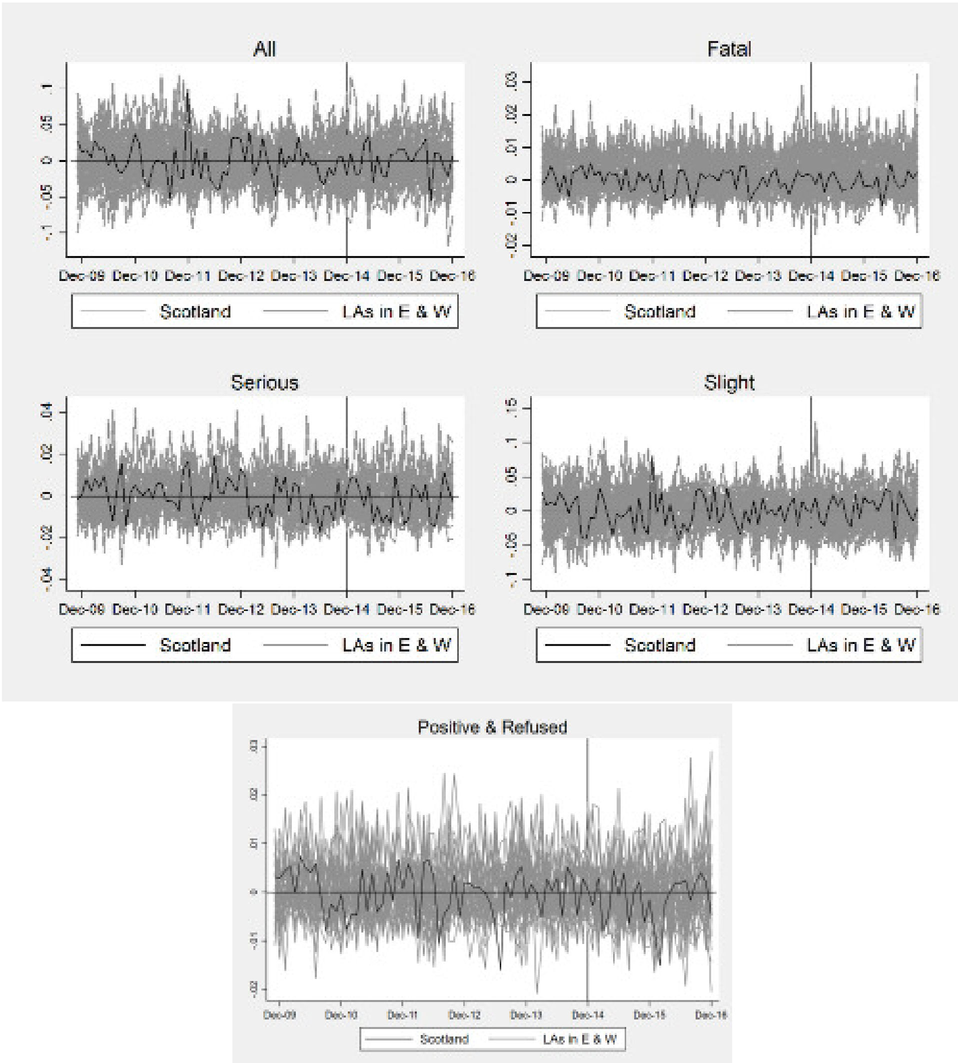


Figure A.8: Gaps in Road Accident Rates for Scotland and Synthetic Scotland and for Scotland and Placebos in Control LAs, by Day of the Week (cont.)

(b) Friday, Saturday & Sunday



Notes: In all panels, placebo districts with pre-reform mean squared prediction error (MSPE) that are two times higher than Scotland's are excluded.

Figure A.9: Gaps in Road Accident Rates for Scotland and Synthetic Scotland and for Scotland and Placebos in Control LAs, by Day of the Week

(a) Aged 18–25

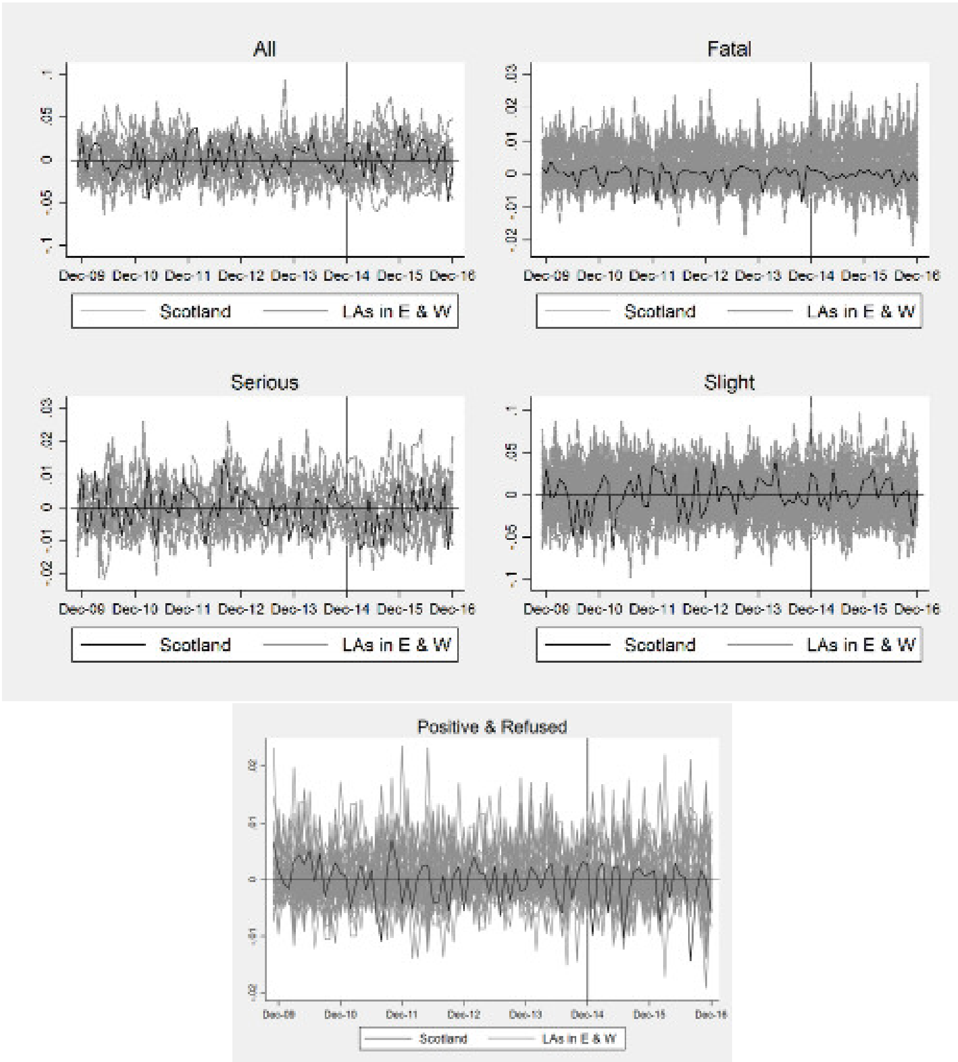


Figure A.9: Gaps in Road Accident Rates for Scotland and Synthetic Scotland and for Scotland and Placebos in Control LAs, by Day of the Week (cont.)

(b) Aged 18–30

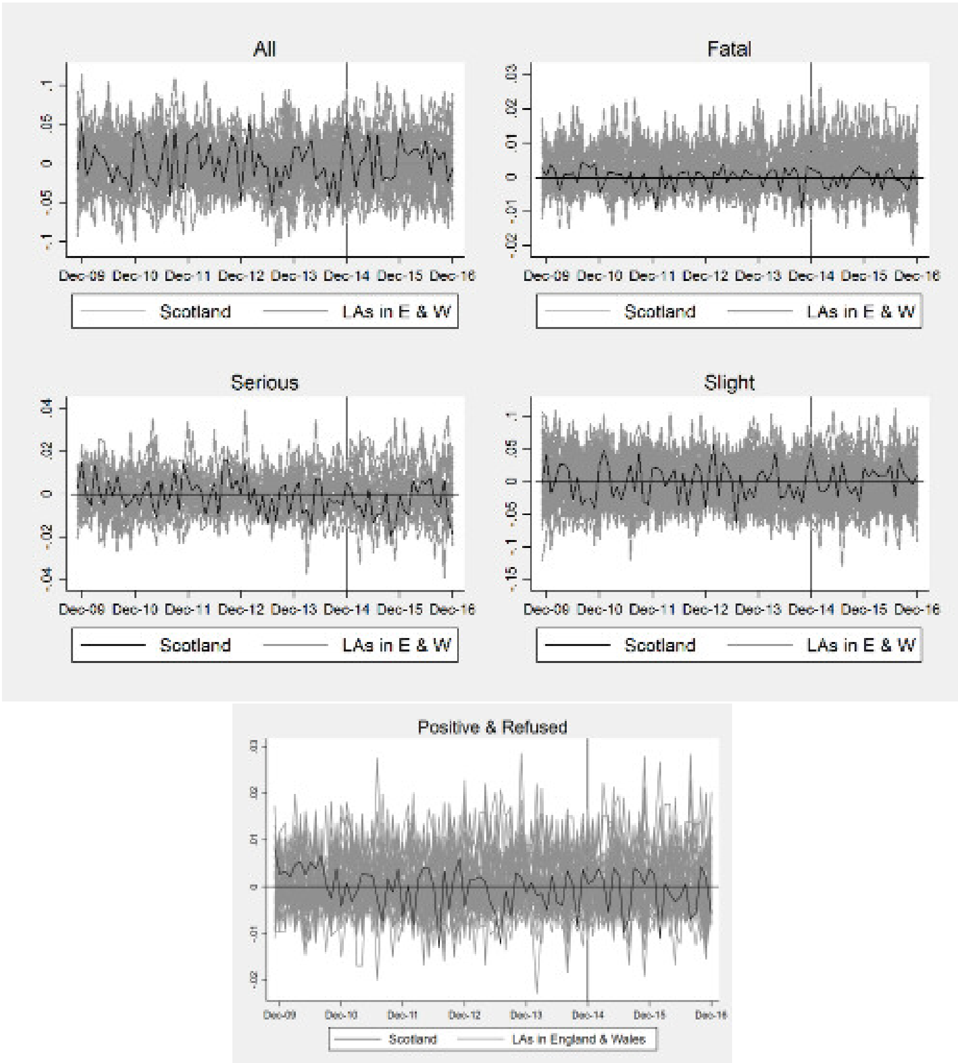
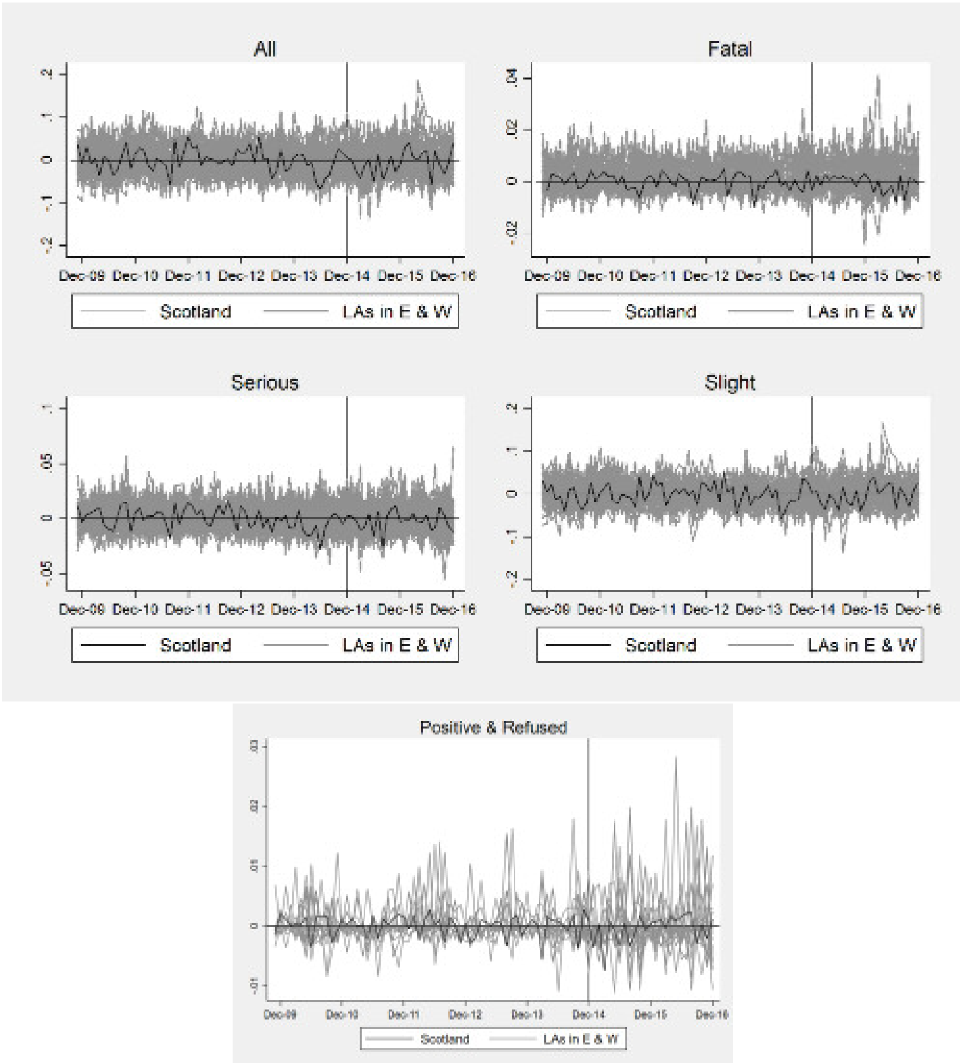


Figure A.9: Gaps in Road Accident Rates for Scotland and Synthetic Scotland and for Scotland and Placebos in Control LAs, by Day of the Week (cont.)

(c) Aged 50+



Notes: In all panels, placebo districts with pre-reform mean squared prediction error (MSPE) that are two times higher than Scotland's are excluded.

Figure A.10: Gaps in Road Accident Rates for Scotland and Synthetic Scotland and for Scotland and Placebos in Control LAs, by Gender

(a) Male

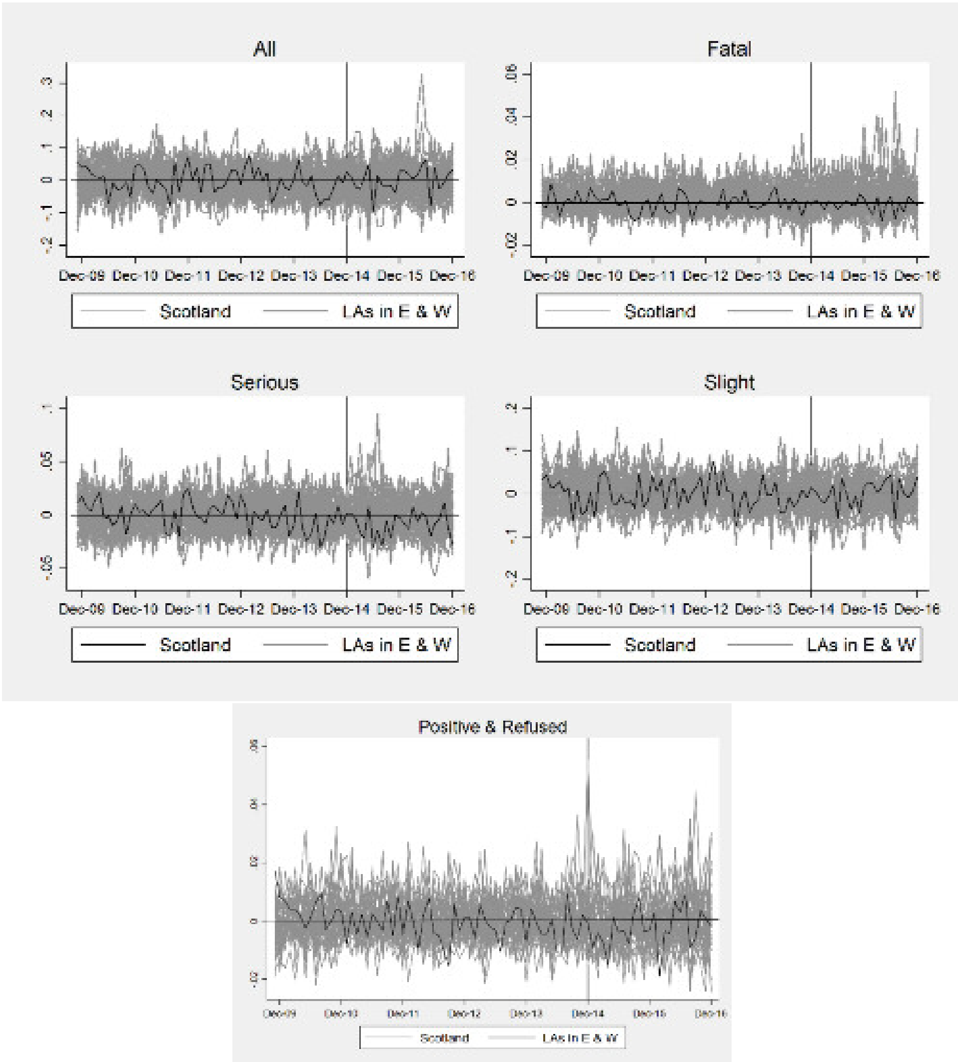
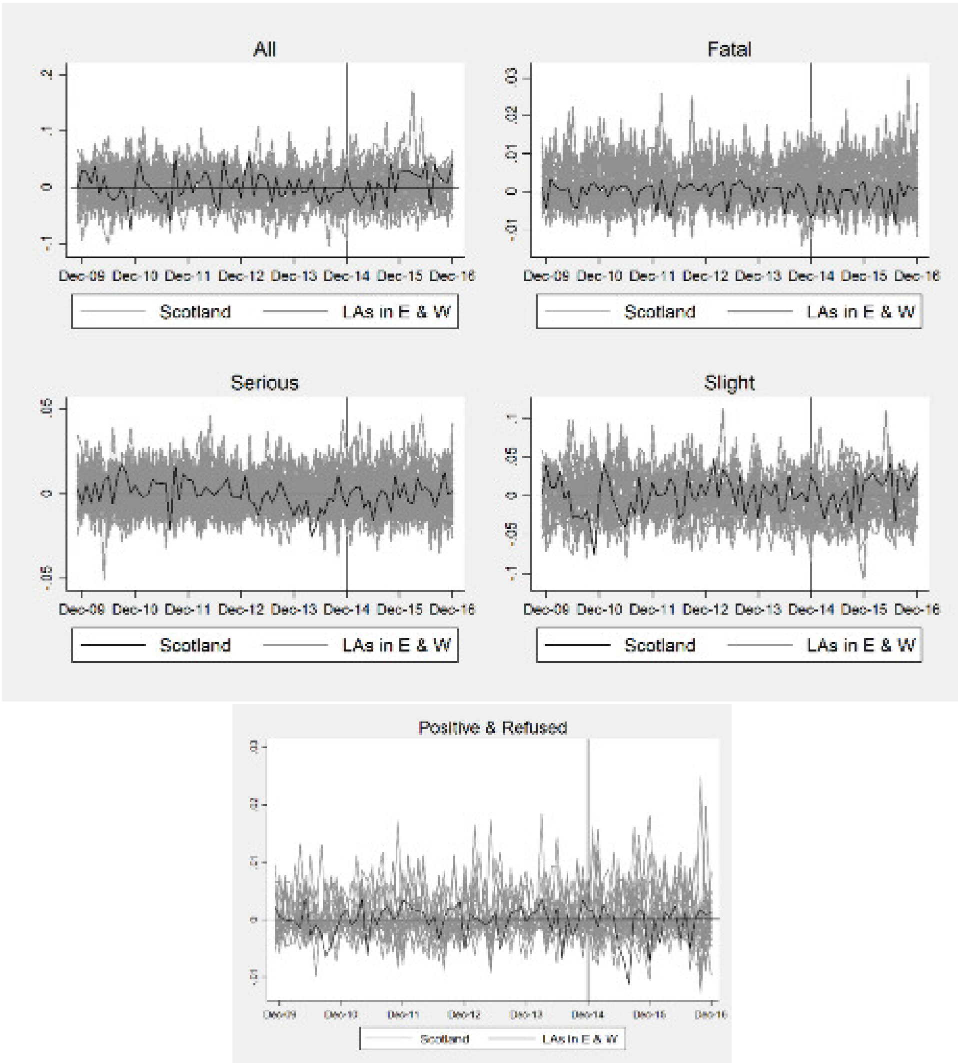


Figure A.10: Gaps in Road Accident Rates for Scotland and Synthetic Scotland and for Scotland and Placebos in Control LAs, by Gender (cont.)

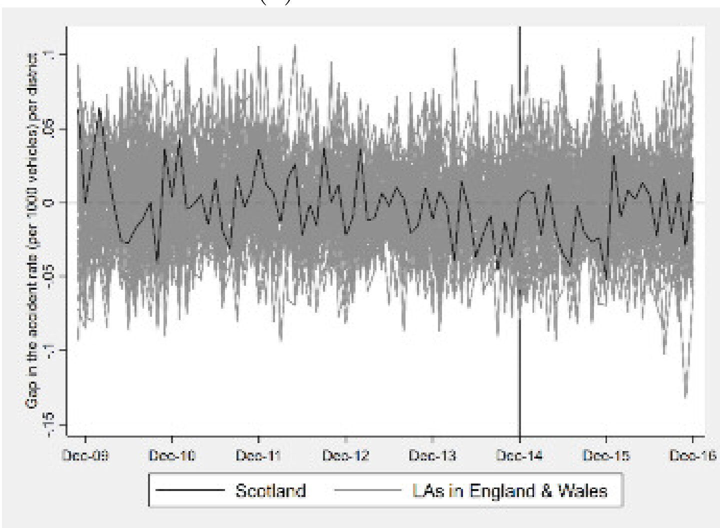
(b) Female



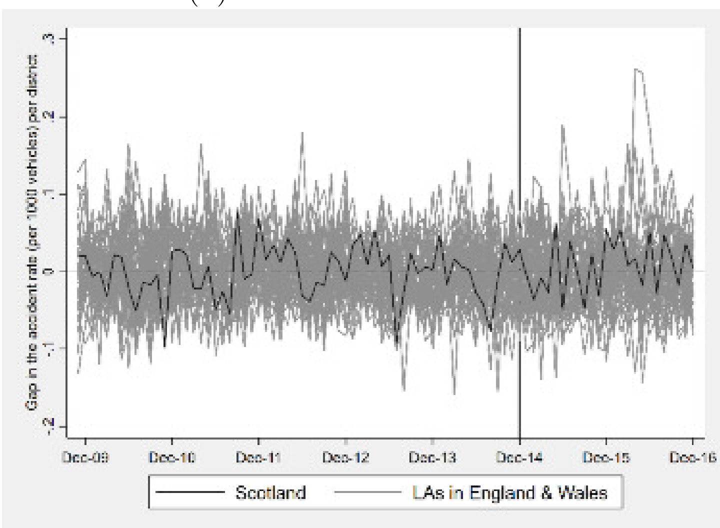
Notes: In all panels, placebo districts with pre-reform mean squared prediction error (MSPE) that are two times higher than Scotland's are excluded.

Figure A.11: Gaps in Road Accident Rates for Scotland and Synthetic Scotland and for Scotland and Placebos in Control LAs, by Number of Vehicles Involved in the Accident

(a) One Vehicle



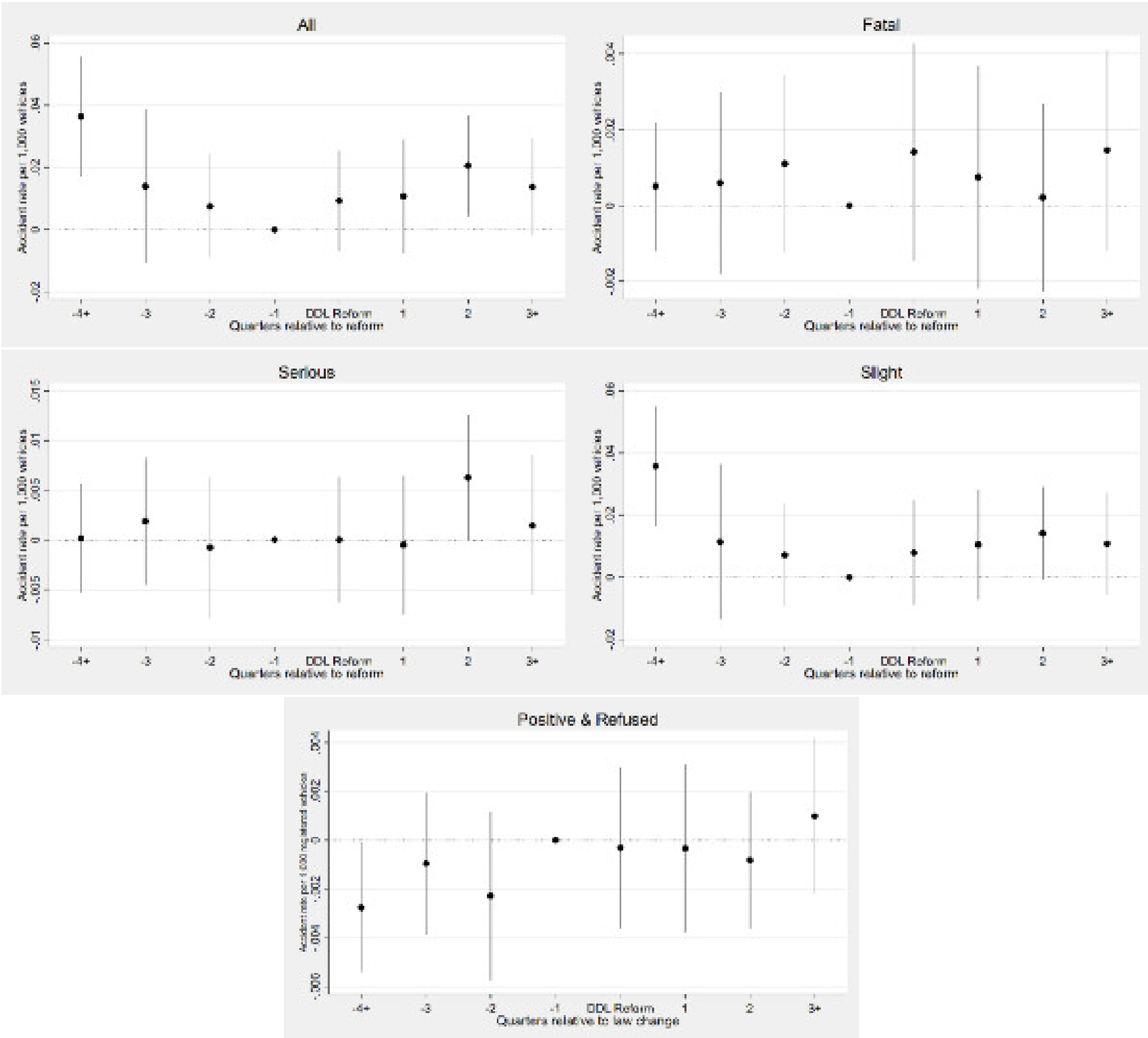
(b) Two or More Vehicles



Notes: In all panels, placebo districts with pre-reform mean squared prediction error (MSPE) that are two times higher than Scotland's are excluded.

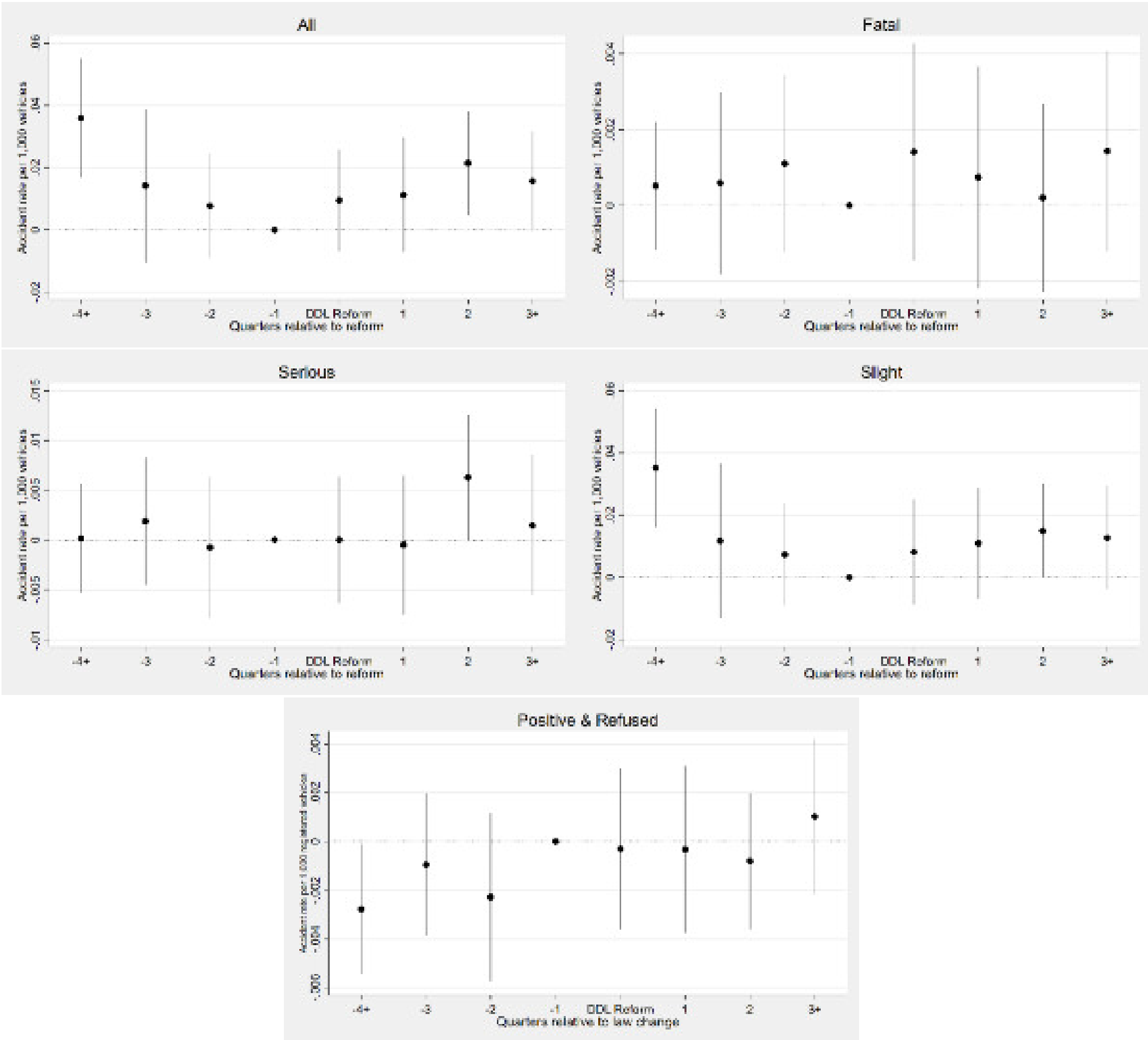
C. Robustness Checks

Figure A.12: Effect of the DDL Reform on Road Accident Rates at Quarters Around the Reform — Event-Study Estimates Without Controlling for Local Unemployment Rate



Notes: Each plot shows treatment effect estimates from a linear panel event-study version of (1), following the procedure proposed by Freyaldenhoven, Hansen, and Shapiro (2019). The vertical bars around each estimate are the 95% confidence intervals. Standard errors are clustered at the local authority level.

Figure A.13: Effect of the DDL Reform on Road Accident Rates at Quarters Around the Reform — Event-Study Estimates Controlling for Local Unemployment Rate



Notes: Each plot shows treatment effect estimates from a linear panel event-study version of (1), following the procedure proposed by Freyaldenhoven, Hansen, and Shapiro (2019). The vertical bars around each estimate are the 95% confidence intervals. Standard errors are clustered at the local authority level.

Table A.1: Effect of the DDL Reform on Drink Drive Accident Rates — Difference-in-Difference Estimates Based on an Alternative Definition of Drink Driving Accidents

	Mean	(a)	(b)	(c)	(d)	(e)	(f)
				A. All			
β	0.157	-0.004	-0.006	0.030	0.025	0.033	0.034
		(0.018)	(0.016)	(0.022)	(0.025)	(0.027)	(0.027)
				B. Fatal			
β	0.006	0.004**	0.004**	0.005*	0.006	0.004	0.005
		(0.002)	(0.002)	(0.003)	(0.004)	(0.003)	(0.004)
				C. Serious			
β	0.025	-0.003	-0.003	0.006	0.006	0.006	0.008*
		(0.004)	(0.003)	(0.004)	(0.005)	(0.004)	(0.005)
				D. Slight			
β	0.126	-0.006	-0.008	0.023	0.017	0.026	0.025
		(0.016)	(0.015)	(0.021)	(0.023)	(0.025)	(0.027)
Observations		77	77	77	77	77	77
Controls		N	Y	Y	Y	Y	Y
Linear annual trend		N	N	Y	N	Y	N
Linear annual × Scotland		N	N	Y	N	Y	N
Linear annual × region		N	N	N	Y	N	Y
Region fixed effects		N	N	N	N	Y	Y

Notes: Observations are at the region-year level. The sample period goes from 2009 until 2016. Robust standard errors are in parentheses. The control regions are: North East, North West, Yorkshire and the Humber, East Midlands, West Midlands, East, South East, London, South West, Wales. Controls are: monthly regional average temperature range, population density, road length, proportion of residents with no qualification, proportion of residents with bad or very bad general health, median hours worked per week, Job Seeker's Allowance rate, and the number of alcohol premises. The definition of a drink-drive accident is a reported incident on a public road in which someone is killed or injured, where at least one of the motor vehicle drivers or riders involved met one of the following criteria: (i) failed a roadside breath test by registering above 35 μg/100 ml of breath (England and Wales) or 22μg/100 ml (Scotland) after December 2014; (ii) refused to give a breath test specimen when requested by the police, other than when incapable of doing so for medical reasons; (iii) died, within 12 hours of the accident, and was subsequently found to have more than 80mg of alcohol per 100ml of blood (England and Wales) or 50mg (Scotland).
* $p < 0.10$, ** $p < 0.05$.

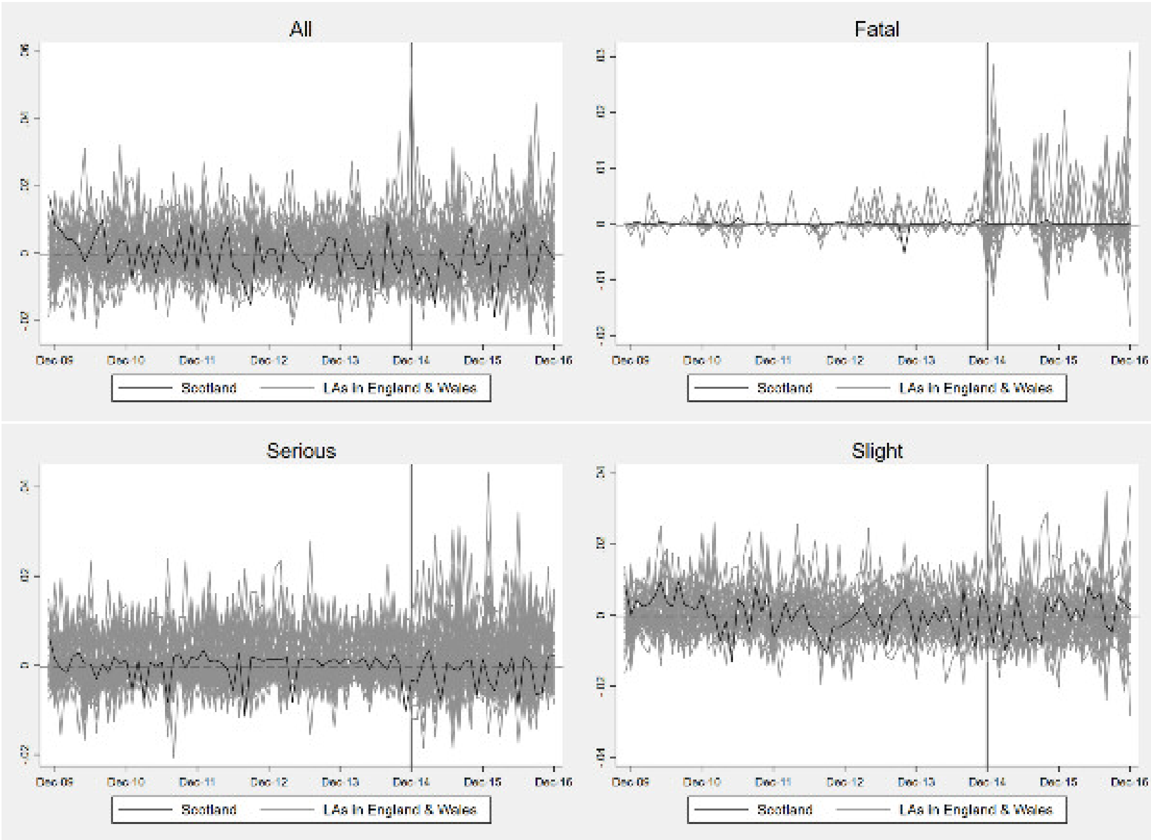
Table A.2: Effect of the DDL Reform on Road Accident Rates with Positive/Refused Breath Test — Difference-in-Difference and Spatial Regression Discontinuity Estimates

	Mean	(a)	(b)	(c)	(d)	(e)	(f) <200km	(g) <100km	(h) <50km
					A. Fatal				
β	0.00011	-0.00008*	-0.00008*	-0.00008	-0.00005	-0.00005	0.000107	0.000200	0.000219
		(0.00005)	(0.00005)	(0.000102)	(0.000101)	(0.000101)	(0.000117)	(0.000119)	(0.000326)
							[0.548]	[0.113]	[0.548]
					B. Serious				
β	0.00169	0.00006	0.00006	0.000475*	0.000408	0.000408	0.000717*	-0.000318	-0.000171
		(0.000243)	(0.000243)	(0.000287)	(0.000311)	(0.000311)	(0.000390)	(0.000595)	(0.000682)
							[0.839]	[0.604]	[0.839]
					C. Slight				
β	0.00688	-0.000438	-0.000438	0.00101	0.00116	0.00116	0.00170*	0.00173	0.00176
		(0.000534)	(0.000534)	(0.000806)	(0.000802)	(0.000802)	(0.000933)	(0.00174)	(0.00512)
							[0.738]	[0.348]	[0.738]
Obersvations		32,164	32,164	32,164	32,164	32,164	8,170	2,236	602
Controls		N	Y	Y	Y	Y	Y	Y	Y
Monthly trend		N	N	Y	Y	Y	Y	Y	Y
Monthly trend × Scotland		N	N	Y	Y	Y	Y	Y	Y
Month of year dummies		N	N	N	Y	Y	Y	Y	Y
Month of year × Scotland		N	N	N	Y	Y	Y	Y	Y
LA fixed effects		N	N	N	N	Y	Y	Y	Y

Notes: Observations are at the LA-month-year level. The dependent variable is the number of accidents with a positive or refused breath test per 1,000 registered vehicles. The sample period goes from November 2009 to December 2016. Standard errors clustered at the LA level are in parentheses. For the spatial regression discontinuity results, due to the small number of LAs, wild bootstrapped p-values computed using Webb weights (Webb, 2014) and 5,000 replications are in square brackets. For completeness, however, these are shown also for large bandwidths. 'LAs' denotes local authorities. 'Mean' refers to the Scottish pre-reform mean of the dependent variable. 'Controls' are monthly averages of temperature range (at the UK Met Office climate region), population density, proportion of residents aged 16 or more with no educational qualification, proportion of residents with bad or very bad health, median total hours worked, median gross pay, Job Seeker's Allowance rate, alcohol licensed premises, and total road length.

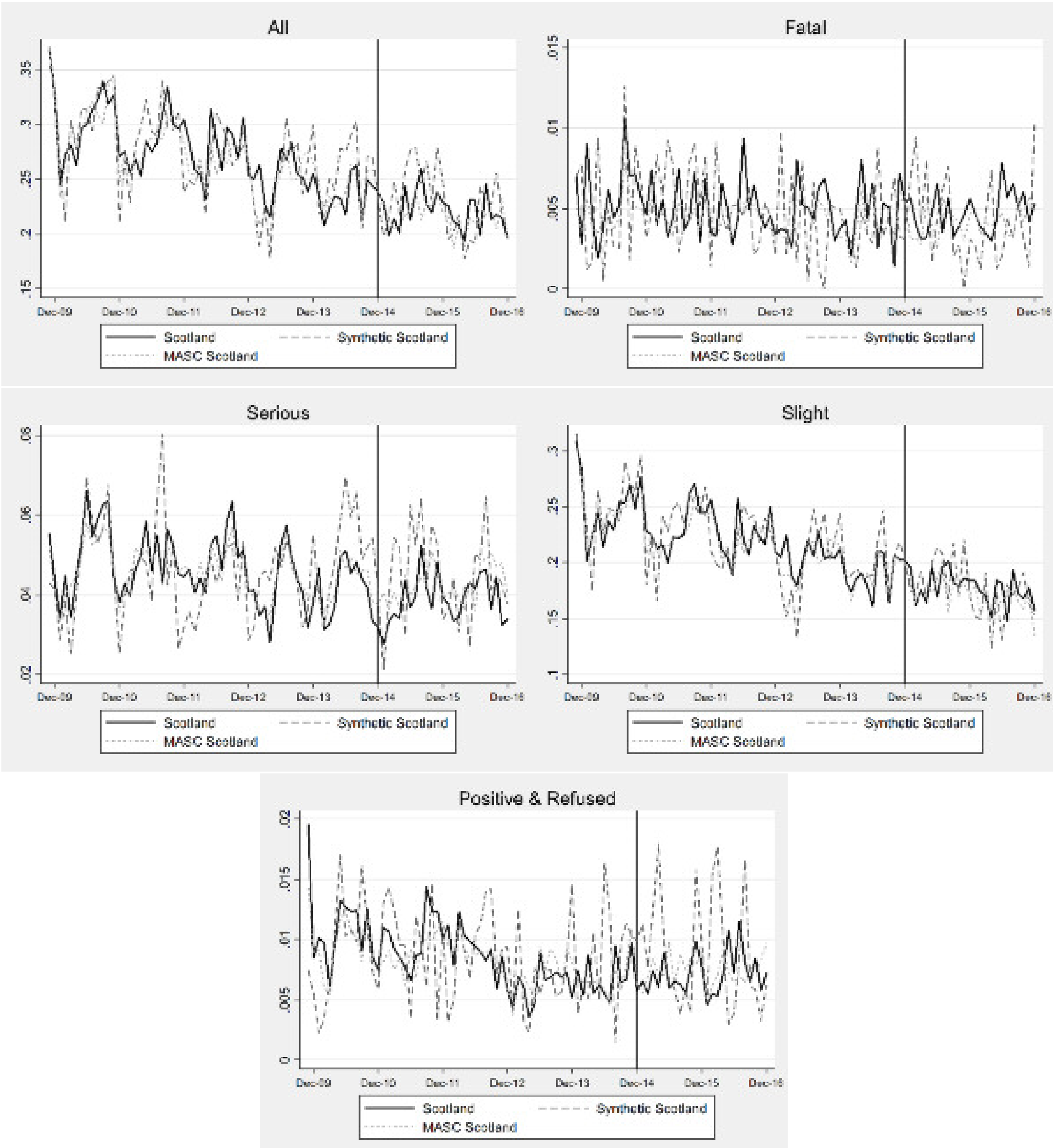
* $p < 0.10$

Figure A.14: Gaps in Road Accident Rates with Positive/Refused Breath Test for Scotland and Synthetic Scotland and for Scotland and Placebos in Control LAs



Notes: In all panels, placebo districts with pre-reform mean squared prediction error (MSPE) that are two times higher than Scotland's are excluded. All panels show results for accident rates with a positive or refused breath test. The panels are all accidents (top left), fatal accidents (top right), serious accidents (bottom left), and slight accidents (bottom right).

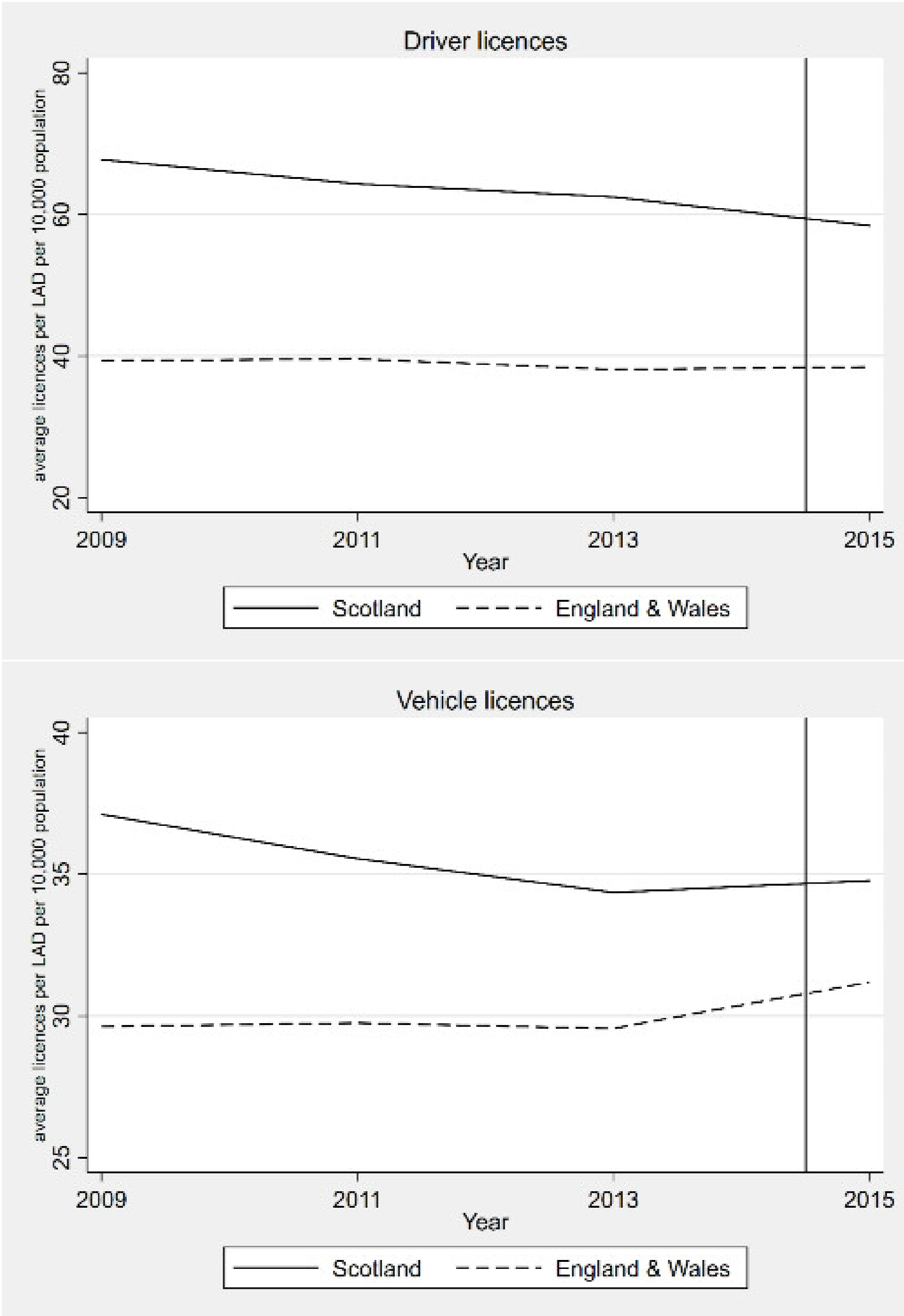
Figure A.15: Trends in Road Accident Rates, by Type: Scotland vs Synthetic Scotland vs MASC Scotland



Notes: 'Synthetic Scotland' calculated using the method of Abadie, Diamond, and Hainmueller (2010). 'MASC Scotland' calculated using the matching and synthetic control approach proposed by Kellogg et al. (2019).

5. Why Was the DDL Reform Ineffective?

Figure A.16: Trends in Taxi Licence Rates: Scotland versus the Rest of Britain



Sources: Department for Transport, Taxi Statistics – Table TAXI0104 “Taxis, Private Hire Vehicles (PHVs) and their drivers”, Taxi Licensing Authorities.

Table A.3: Effect of the DDL Reform on Taxi Licence Rates — Spatial Regression Discontinuity Estimates

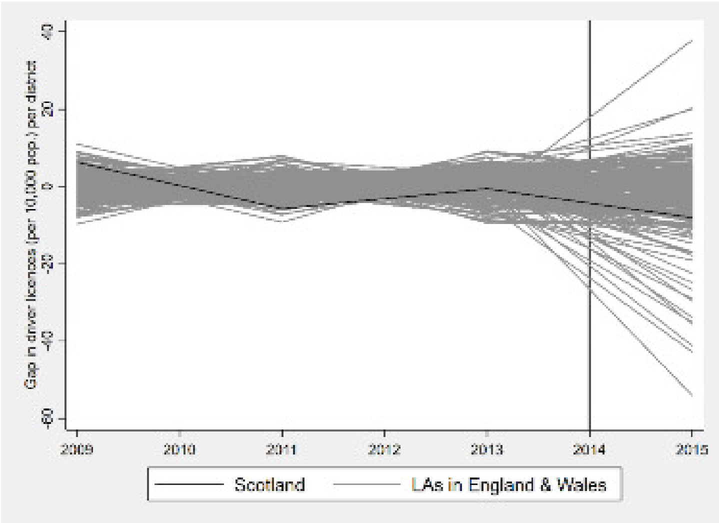
	(a) <200 km	(b) <100 km	(c) <50 km
	A. Driver Licences per 10,000 Pop.		
β	-4.465	-0.739	-3.535
	(4.511)	(3.068)	(7.094)
	[0.373]	[0.819]	[0.609]
Mean	62.4	57.7	41.9
	B. Vehicle Licences per 10,000 Pop.		
β	-1.963	1.278	1.073
	(2.703)	(1.935)	(5.183)
	[0.652]	[0.539]	[0.871]
Mean	36.0	31.5	26.3
Observations	384	104	28
Number of LAs	96	26	7

Sources: Department for Transport, Taxi Statistics – Table TAXI0104; Office for National Statistics.

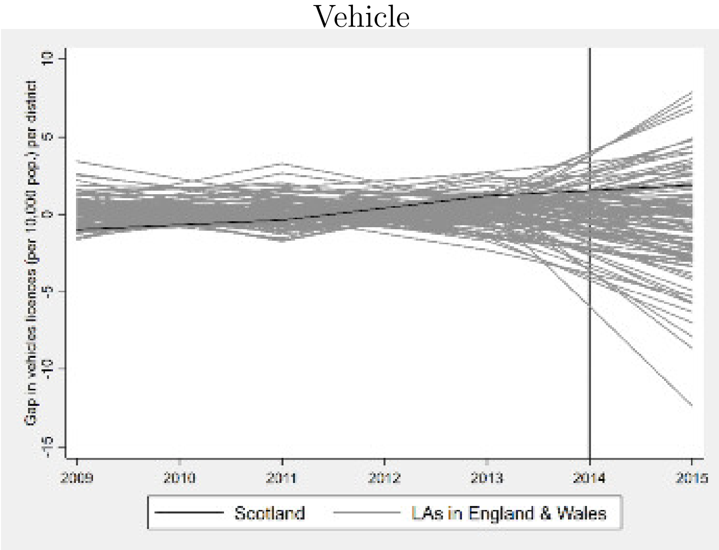
Notes: Observations at the LA-year level. The sample period goes from 2009 to 2015 (biennially). Standard errors are clustered at the LA level. 'Mean' refers to the Scottish pre-reform mean of the dependent variable. 'Controls' are LA monthly averages of temperature range, population density, proportion of residents aged 16 or more with no educational qualification, proportion of residents with bad or very bad health, median total hours worked, median gross pay, Job Seeker's Allowance rate, alcohol licensed premises, and total road length. Due to the small number of LAs, wild bootstrapped p-values computed using Webb weights (Webb, 2014) and 5,000 replications are in square brackets. For completeness, however, these are shown also for large bandwidths. 'LAs' denotes local authorities.

Figure A.17: Gaps in Taxi Licence Rates for Scotland and Synthetic Scotland and for Scotland and Placebos in Control LAs, by Type of License

Driver

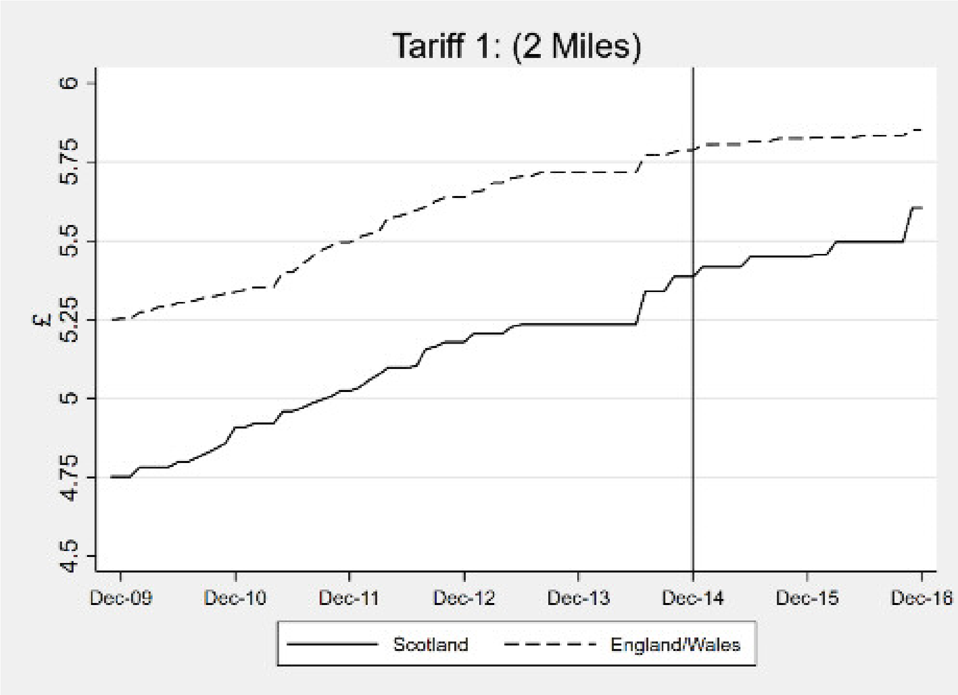


Vehicle



Notes: In both panels, placebo districts with pre-reform mean squared prediction error (MSPE) that are two times higher than Scotland's are excluded.

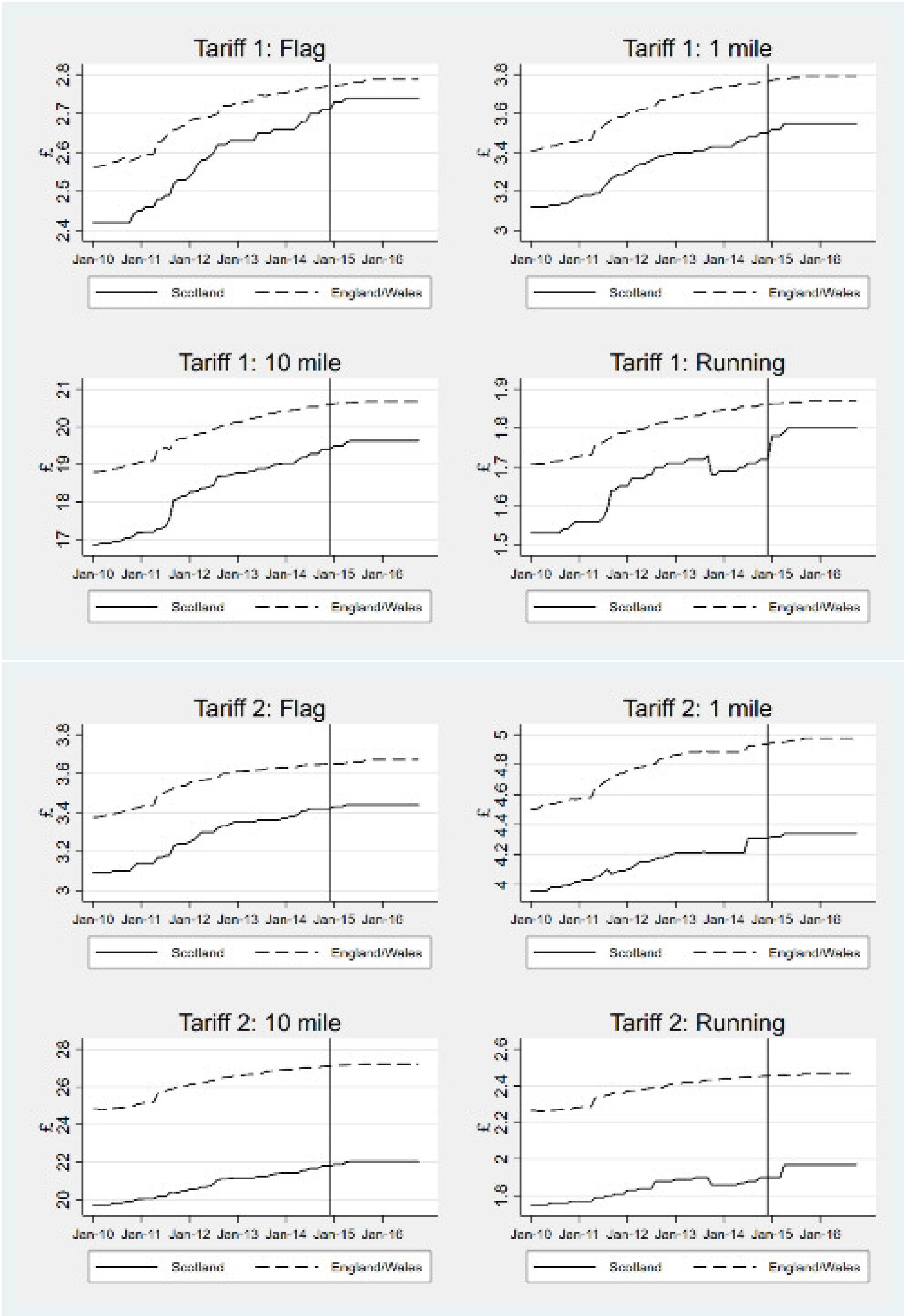
Figure A.18: Trends in Taxi Tariffs: Scotland versus the Rest of Britain



Source: Private Hire and Taxi Monthly, <`https://www.phtm.co.uk/taxi-fares-league-tables`>.

Notes: Data at the local authority-month level. Tariff 1 normally corresponds to a day-rate, and the figures are monthly average tariffs for 2-mile journeys.

Figure A.19: Trends in Taxi Tariffs: Scotland versus the Rest of Britain



Source: Private Hire and Taxi Monthly, <`https://www.phtm.co.uk/taxi-fares-league-tables`>. *Notes*: Data are at regional (7 regions)-month level. Tariff 1 normally corresponds to a day-rate, and Tariff 2 corresponds to a night rate. Figures are monthly average tariffs for hailing a cab (or flag fare), the average tariffs of 1- and 10-mile journeys, and the mean charge per mile travelled after the initial pull-off distance (or running mile fare).

Table A.4: Effect of the DDL Reform on Taxi Tariffs — Difference-in-Difference Estimates

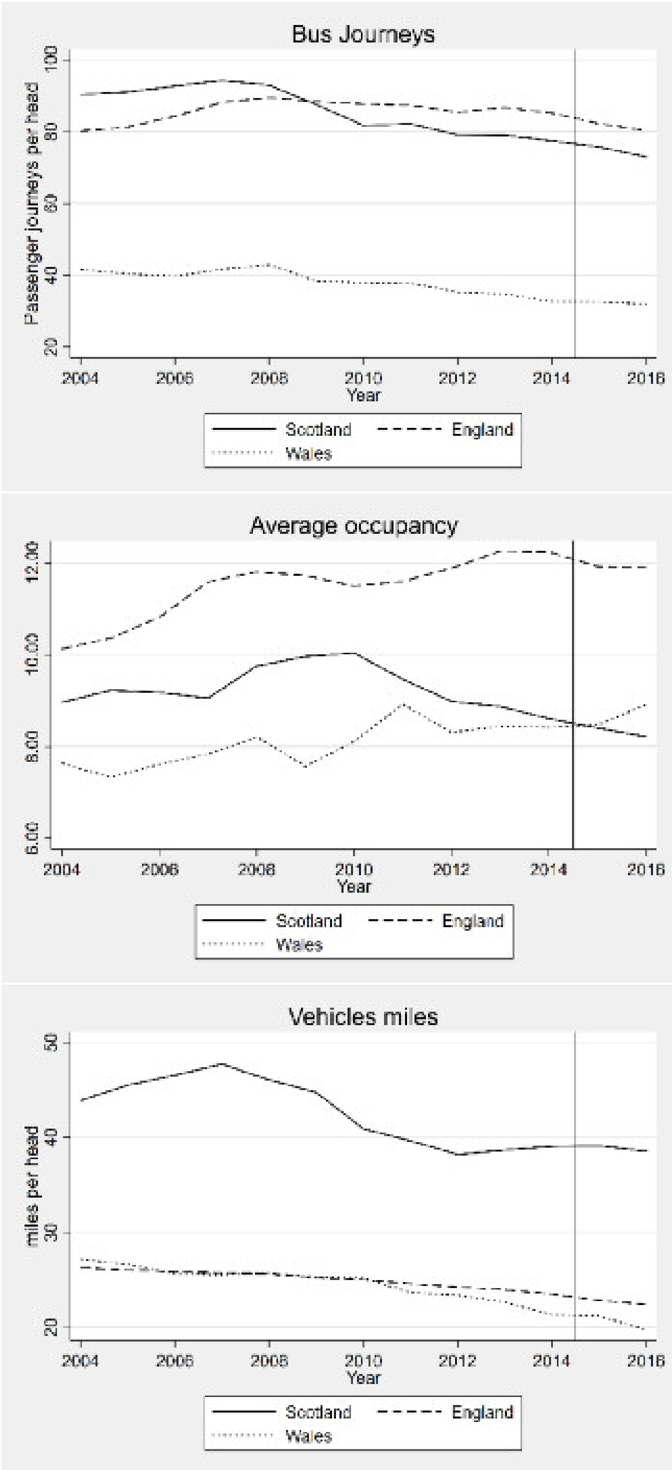
	Mean	(a)	(b)	(c)	(d)
		A. Tariff 1			
Flag	2.54	0.016	-0.017	-0.018	-0.016
		(0.019)	(0.016)	(0.016)	(0.016)
1 mile	3.28	0.023	-0.011	-0.014	-0.011
		(0.024)	(0.020)	(0.021)	(0.020)
10-mile	18.02	0.113	-0.043	0.066	0.132
		(0.135)	(0.127)	(0.126)	(0.122)
Running	1.63	0.022	0.0002	0.010	0.011
		(0.016)	(0.015)	(0.015)	(0.014)
		B. Tariff 2			
Flag	3.24	0.018	-0.019	-0.005	-0.001
		(0.025)	(0.023)	(0.024)	(0.023)
1 mile	4.11	0.042	0.0002	0.016	0.017
		(0.030)	(0.026)	(0.027)	(0.026)
10-mile	20.60	0.173	-0.013	0.091	0.122
		(0.148)	(0.134)	(0.134)	(0.130)
Running	1.82	0.027*	0.008	0.010	0.013
		(0.016)	(0.013)	(0.013)	(0.013)
Observations		658	658	658	658
Month-year trend		N	Y	Y	Y
Month-year trend × Scotland		N	Y	Y	Y
Month FEs		N	N	Y	Y
Month FEs × Scotland		N	N	Y	Y
Region fixed effects		N	N	N	Y

Source: Private Hire and Taxi Monthly, <`https://www.phtm.co.uk/taxi-fares-league-tables`>.

Notes: Observations are at the region-month-year level. The sample period goes from January 2009 to October 2016. All regressions include regional indicators, where the regions are: East Anglia, Midlands, North, South, South West, and Wales. Panel-corrected standard errors are calculated using a Prais-Winsten regression, where a region-specific AR(1) process is assumed. This also allows the error terms to be region specific, heteroskedastic, and contemporaneously correlated across regions. 'Mean' refers to the Scottish pre-reform mean of the dependent variable. 'FEs' denotes fixed effects. For other definitions, see the notes to Figure A.19.

* $p < 0.10$.

Figure A.20: Trends in Bus Availability, by Measure of Bus Usage



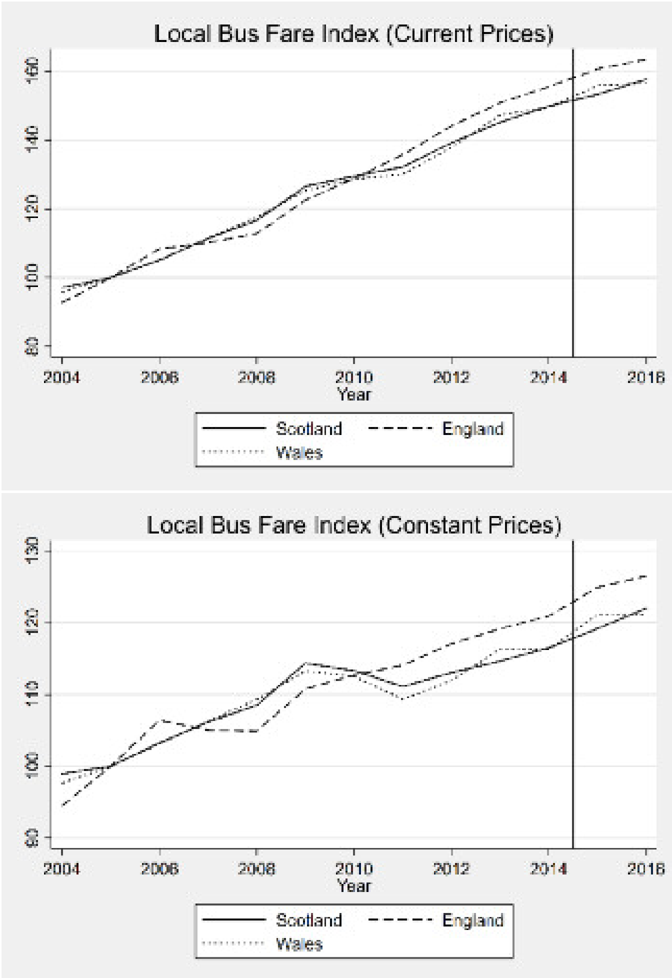
Source: Department for Transport Bus Statistics (Tables BUS0108, BUS0206, BUS0304).

Table A.5: Effect of the DDL Reform on Bus Journeys — Difference-in-Difference Estimates

	(a)	(b)	(c)	(d)
	2014/2015		2015/2016	
	A. Per Capita Bus Journeys			
β	-7.109	3.996	-6.898	3.552
	(15.02)	(24.69)	(16.58)	(24.03)
Mean	87.1		86.2	
	B. Average Bus Occupancy			
β	-0.712*	-0.481	-0.468	-0.123
	(0.404)	(0.341)	(0.428)	(0.460)
Mean	9.35		9.28	
	C. Per Capita Bus Miles			
β	1.349	1.439	1.077	0.982
	(1.366)	(1.585)	(1.414)	(1.599)
Mean	224.9		223.5	
Observations	39	39	39	39
Linear annual Trend	N	Y	N	Y
Linear annual Trend × Scotland	N	Y	N	Y

Source: Department for Transport Bus Statistics (Tables BUS0108, BUS0206, BUS0304). *Notes*: Observations are at the country-year-level. The sample period goes from 2004/05 to 2016/17. Control countries are England and Wales (separately). The dependent variables are: average passenger journeys on local bus services by region per head of population, annual from 1991/92 (panel A); average bus occupancy on local bus services (panel B); vehicle kilometres per head on local bus services (panel C). Panel-corrected standard errors are calculated using a Prais-Winsten regression, where a region-specific AR(1) process is assumed. This also allows the error terms to be region specific, heteroskedastic, and contemporaneously correlated across regions. 'Mean' refers to the Scottish pre-reform mean of the dependent variable.
* $p < 0.10$.

Figure A.21: Trends in Bus Fares



Source: Department for Transport Bus Statistics (Tables BUS0405a, BUS0405b).
Notes: Fare figures are in pence. The constant price fares are expressed in 2005 prices.

Table A.6: Effect of the DDL Reform on Bus Fares — Difference-in-Difference Estimates

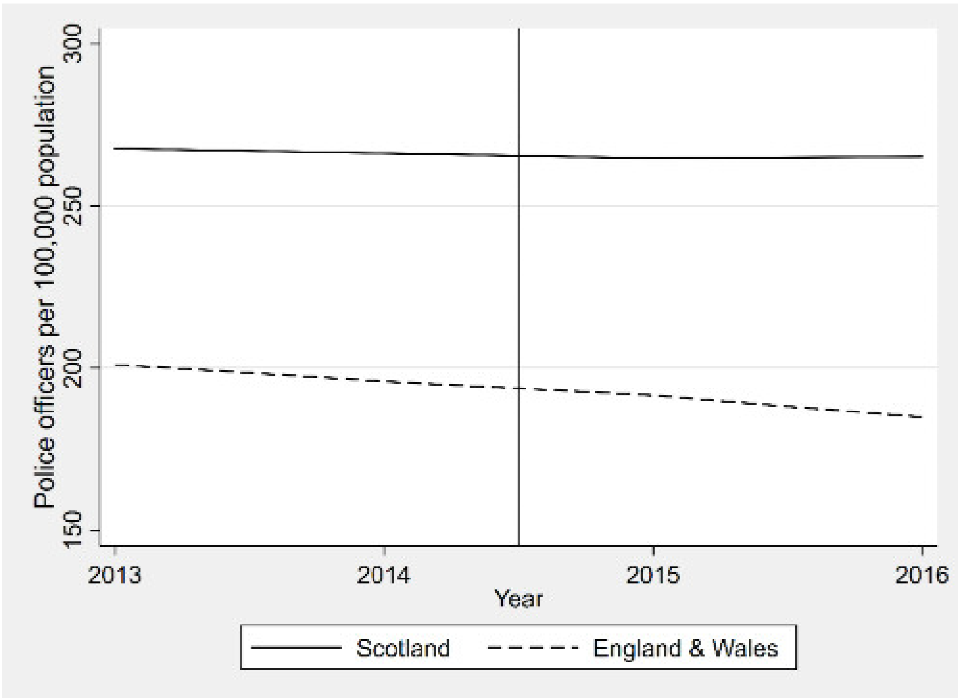
	(a)	(b)	(c)	(d)
	2014/2015		2015/2016	
	A. Current Prices			
β	-2.401	-1.230	-2.450	-1.875
	(1.819)	(1.416)	(1.567)	(1.599)
Mean	120.3		123.0	
	B. Constant Prices			
β	-2.148	-0.737	-2.092	-0.661
	(1.438)	(1.910)	(1.388)	(1.508)
Mean	108.3		109.1	
Observations	39	39	39	39
Linear annual Trend	N	Y	N	Y
Linear annual Trend × Scotland	N	Y	N	Y

Source: Department for Transport Bus Statistics (Tables BUS0108, BUS0206, BUS0304).

Notes: Observations are at the country-year-level. The sample period goes from 2004/05 to 2016/17. Control countries are England and Wales (separately). The dependent variables are: local bus fares at current prices (panel A); local bus fares at constant prices (panel B). Panel-corrected standard errors are calculated using a Prais-Winsten regression, where a region-specific AR(1) process is assumed. This also allows the error terms to be region specific, heteroskedastic, and contemporaneously correlated across regions.

Police Numbers — Due to restructuring of the police force in Scotland and to changes in recording police officer activities in both Scotland and the rest of Britain around the 2014 BAC reform, we can only examine police numbers overall and not the number of police officers deployed in specific activities, such as traffic duties. Police Scotland was formed in April 2013. There were originally 14 divisions: Aberdeen City, Aberdeenshire and Moray, Tayside, Highland and Islands, Forth Valley, Edinburgh, The Lothians and Scottish Borders, Fife, Glasgow, Ayrshire, Lanarkshire, Argyll and West Dunbartonshire, Renfrewshire and Inverclyde, and Dumfries and Galloway. By March 2016 there were 13 as Aberdeen City and Aberdeenshire and Moray had merged into North East. Police Scotland stopped reporting the number of police officers assigned to the road policing unit in quarter ending on 31 March 2014. Local police officer resources are the core compliment of officers under the direction of the local commander and include community policing, response policing, and divisional road policing teams. In addition, in England and Wales in 2016 the Home Office changed the definitions of different officer duties making the comparison with earlier years difficult.

Figure A.22: Trends in Police Numbers: Scotland versus the Rest of Britain



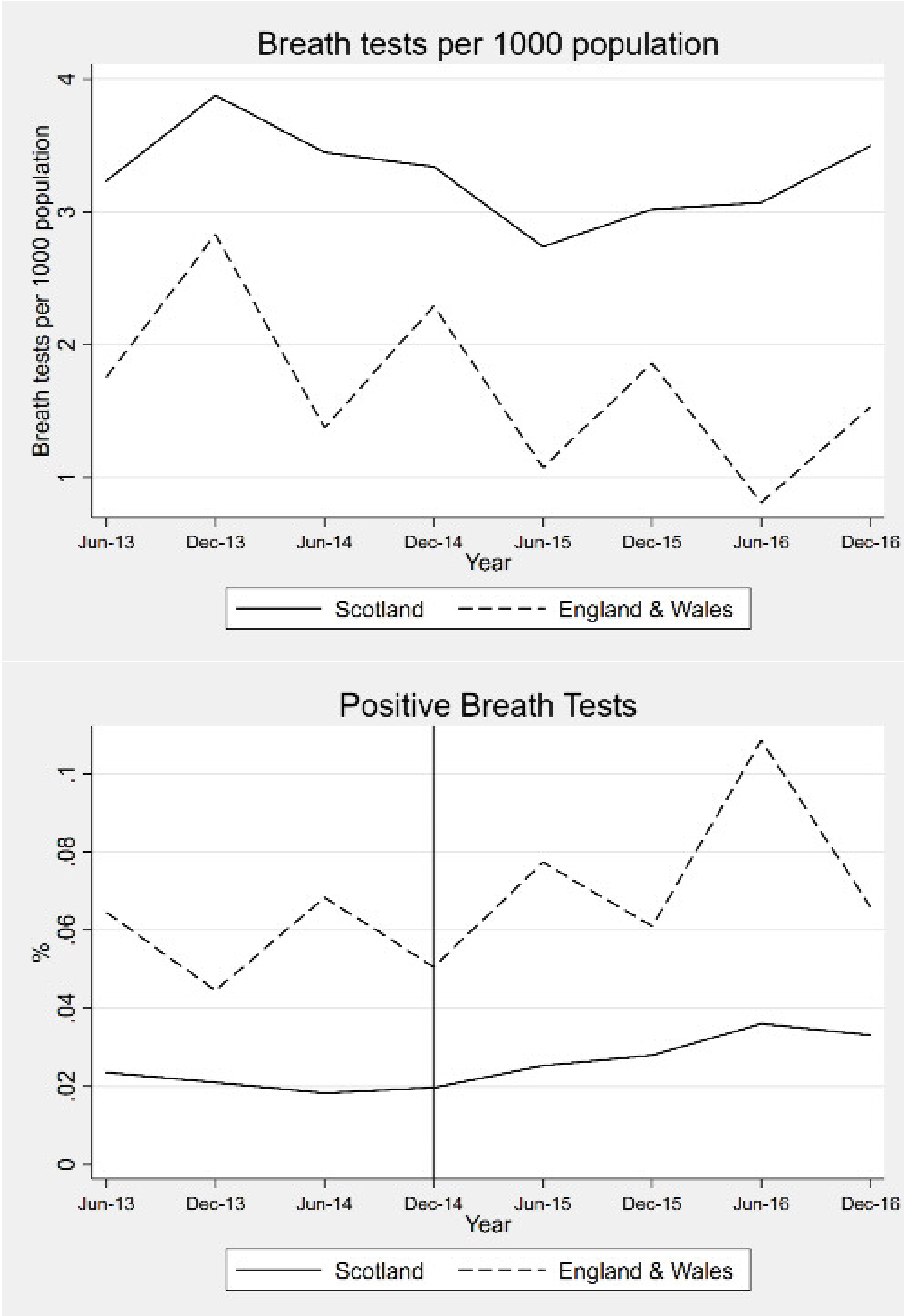
Sources: Police Officer Quarterly Strength Statistics (Scotland); Home Office Police workforce (England and Wales).

Table A.7: Effect of the DDL Reform on Police Numbers — Difference-in-Difference Estimates

	Mean	(a)	(b)	(c)	(d)
β	267.0	8.226***	18.93*	3.227	-3.032
		(1.950)	(10.21)	(12.73)	(3.381)
Observations		220	220	220	220
Controls		N	Y	Y	Y
Linear annual trend		N	N	Y	Y
Linear annual trend × Scotland		N	N	Y	Y
PFAs fixed effects		N	N	N	Y

Sources: Police Officer Quarterly Strength Statistics (Scotland); Home Office Police workforce (England and Wales).
Notes: Observations are at the PFA-year level. There are 42 PFAs in England and Wales, and 13 regional PFAs in Scotland. The sample period goes from 2013 to 2016. The dependent variables is the number of police officers per 100,000 of the population. Standard errors in parentheses are clustered at the PFA level. 'Mean' refers to the Scottish pre-reform mean of the dependent variable. 'Controls' are yearly averages of temperature range, population density, proportion of residents aged 16 or more with no educational qualification, proportion of residents with bad or very bad health, median total hours worked, and median gross pay, Job Seekers' Allowance rate, alcohol licensed premises, and total road length (see the notes to Table 1 for more details). 'PFAs' denotes police force areas.
* $p < 0.10$, *** $p < 0.01$.

Figure A.23: Trends in Breath Testing: Scotland versus the Rest of Britain



Sources: Scotland: Parliamentary Advisory Council for Transport Safety (PACTS), <`www.tinyurl.com/pacts-breath`>. England and Wales: Home Office Breath test statistics: Police Powers and Procedures.

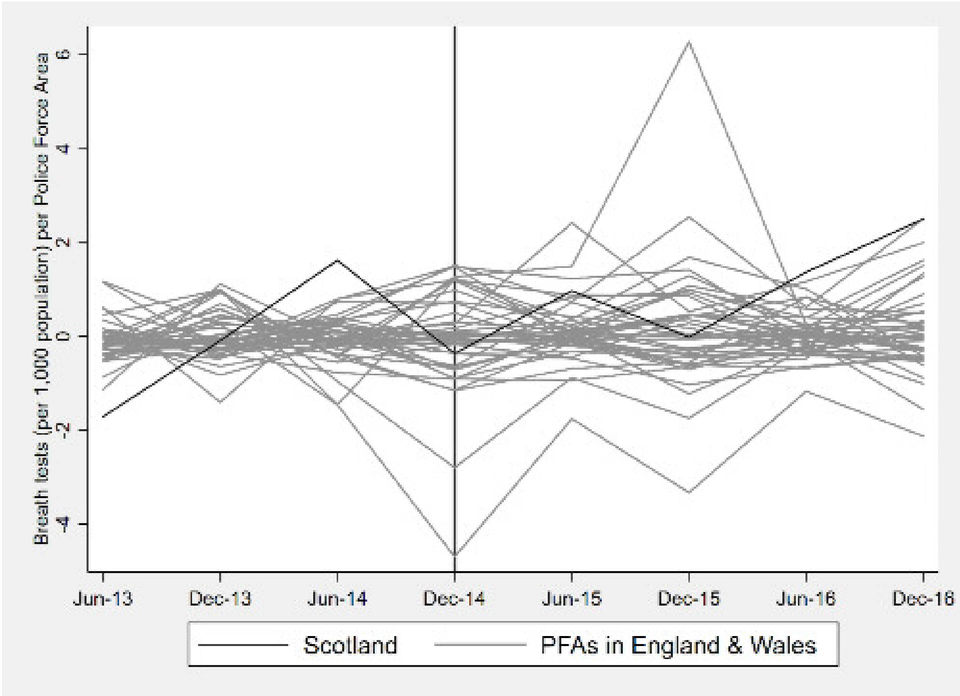
Notes: Observations are at the country-(4-week)-year level. There are two four-week periods per year corresponding to the when Police Scotland undertake their drink driving campaigns. The Scottish campaigns were: Summer 2013 (June, 4 weeks), Festive 2013 (December, 4 weeks), Summer 2014 (June, 2 weeks), Festive 2014 (December, 4 weeks), Summer 2015 (June, 2 weeks), Festive 2015 (December, 4 weeks), Summer 2016 (June, 2 weeks), Festive 2016 (December, 4 weeks). Two week campaigns are scaled up to their four-week equivalent. English and Welch police force data are combined and scaled down to four weeks (28 days) to be comparable to the Scottish data. The sample period goes from 2013 to 2016.

Table A.8: Effect of the DDL Reform on Breath Testing — Difference-in-Difference Estimates

	Mean	(a)	(b)	(c)	(d)	(e)
		A. Breath Tests Administered per 1,000 Population				
β	3.52	0.598	0.890*	-0.783***	-0.828***	-0.759**
		(0.412)	(0.465)	(0.206)	(0.251)	(0.351)
		B. % Positive Breath Tests				
β	0.021	-0.007	-0.013*	-0.005	-0.007	0.0004
		(0.006)	(0.007)	(0.006)	(0.007)	(0.011)
Observations		352	352	352	352	352
Controls		N	Y	Y	Y	Y
Linear time trend		N	N	Y	Y	Y
Linear time trend × Scotland		N	N	Y	Y	Y
June		N	N	N	Y	Y
June × Scotland		N	N	N	Y	Y
PFAs fixed effects		N	N	N	N	Y

Source: Parliamentary Advisory Council for Transport Safety (PACTS), <`www.tinyurl.com/pacts-breath`> (Scotland); Home Office Breath Test Statistics: Police Powers and Procedures (England and Wales).
Notes: Observations are at the country-(4-week)-year level. There are two four-week periods per year corresponding to the when Police Scotland undertake their drink driving campaigns. The Scottish campaigns were: Summer 2013 (June, 4 weeks), Festive 2013 (December, 4 weeks), Summer 2014 (June, 2 weeks), Festive 2014 (December, 4 weeks), Summer 2015 (June, 2 weeks), Festive 2015 (December, 4 weeks), Summer 2016 (June, 2 weeks), Festive 2016 (December, 4 weeks). Two week campaigns are scaled up to their four-week equivalent. England and Wales combined are the control country. English and Welch police force data are combined and scaled down to four weeks (28 days) to be comparable to the Scottish data. The sample period goes from 2013 to 2016. The dependent variables are the number of breath tests administered per 1,000 heads of population (panel A) and the proportion (in percent) of tests that are positive (panel B). 'Mean' refers to the Scottish pre-reform mean of the dependent variable. Robust standard errors in parentheses. 'PFAs' denotes police force areas.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure A.24: Gaps in Breath Testing for Scotland and Synthetic Scotland and for Scotland and Placebos in Control PFAs



Note: Placebo districts with pre-reform mean squared prediction error (MSPE) that are two times higher than Scotland's are excluded.

Figure A.25: Police Scotland's Response to the Request Regarding Drink Drive Arrests

Our Ref: IM-FOI-2019-1654
Date: 10th July 2019

POLICE SCOTLAND
Keeping people safe
POILEAS ALBA

FREEDOM OF INFORMATION (SCOTLAND) ACT 2002

I refer to your recent request for information which has been handled in accordance with the Freedom of Information (Scotland) Act 2002.

For ease of reference, your request is replicated below together with the response.

How many arrests were there for suspicion of drink driving by month from January 2010 to December 2017?

How many of the above were:

- **a. Released without charge**
- **b. Charged?**

Firstly, I must advise you that Police in Scotland have the power to arrest an individual where there is sufficient evidence to support a charge against them - either for a common law offence or for a statutory offence where the statute empowers the police to arrest any person contravening its provisions.

There is however no mandatory recording process in relation to arrests as not all offenders are routinely arrested when they commit offences and some may be subject of a report to the Procurator Fiscal without ever having been arrested

As such, in terms of Section 17 of the Freedom of Information (Scotland) Act 2002 I can confirm that the information you seek is not held by Police Scotland.

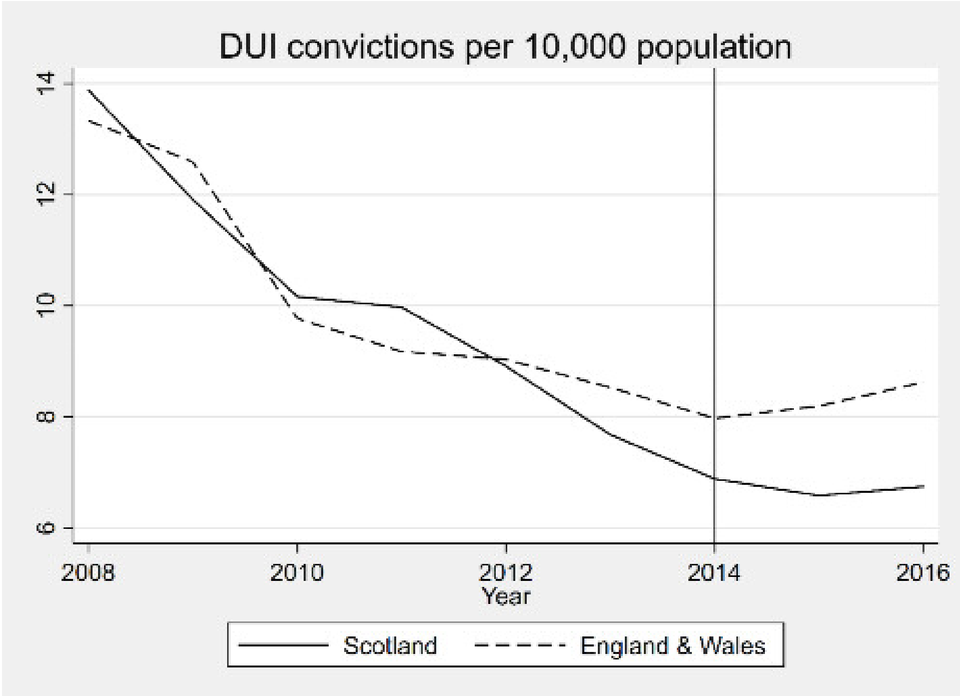
Police Scotland do however record the number of reported and detected crimes and statistics regarding drink driving offences are publicly available on the Police Scotland website:-

https://www.scotland.police.uk/assets/pdf/138327/232757/Police-Scotland-Recorded-and-Detected-Crime-Data

Should you require any further assistance please contact Information Management - Dundee on 01382 596657 quoting the reference number given.

If you are dissatisfied with the way in which Police Scotland has dealt with your request, you are entitled, in the first instance, to request a review of our actions and decisions.
Your request must specify the matter which gives rise to your dissatisfaction and it must be submitted within 40 working days of receiving this response - either by email to foi@scotland.pnn.police.uk or by post to Information Management (Disclosure), Police Scotland, Clyde Gateway, 2 French Street, Dalmarnock, G40 4EH.

Figure A.26: Trends in Drink Drive Conviction Rates: Scotland versus the Rest of Britain



Sources: Criminal Proceedings in Scotland (Scotland). Ministry of Justice (England and Wales). *Notes*: Figures for Scotland cover convictions for drink driving which refers to: (i) driving or in charge of motor vehicle while unfit through drink or drugs; (ii) blood alcohol content above the limit; and (iii) failing to provide breath, blood or urine specimens. Figures for England cover convictions for the following: (i) driving with alcohol in the blood above the prescribed limit; (ii) drive a motor vehicle with the proportion of specified controlled drug above specified limit; (iii) driving and failing to provide specimen for analysis (breath, blood or urine); (iv) in charge of motor vehicle with alcohol in the blood above the prescribed limit; (v) in charge of a motor vehicle with the proportion of specified controlled drug above specified limit; (vi) in charge of motor vehicle while unfit through drink or drugs (impairment); (vii) in charge of motor vehicle and failing to provide specimen for analysis (breath, blood, or urine); (viii) in charge of a vehicle whilst unfit to drive through drink or drugs (impairment); (ix) driving or attempting to drive a vehicle/motor vehicle whilst unfit to drive through drink or drugs; (x) failing to provide specimen for initial breath test; (xi) Failing to allow specimens of blood to be subjected to laboratory test.

Table A.9: Effect of the DDL Reform on Drink Drive Conviction Rates — Difference-in-Difference Estimates

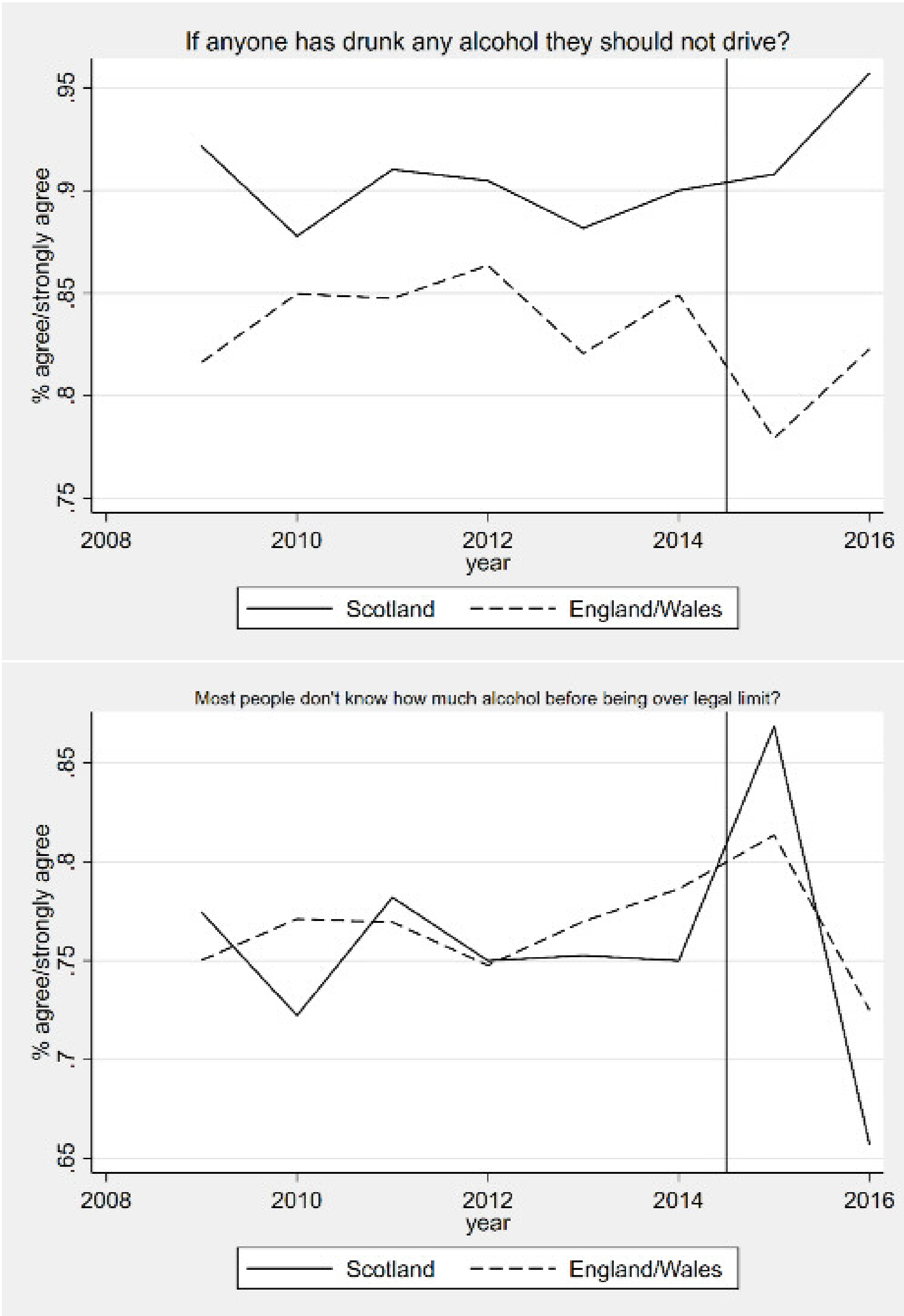
	Mean	(a)	(b)
β	10.95	-1.968**	-0.629
		(0.784)	(0.658)
Linear annual trend		N	Y
Linear annual $\times$ Scotland		N	Y
Observations		18	18

Sources: Criminal Proceedings in Scotland (Scotland); Ministry of Justice (England).
Note: Observations are at the country-year-level. The dependent variable is the number of drink drive convictions per 10,000 population. The sample period goes from 2008 to 2016. Panel-corrected standard errors are calculated using a Prais-Winsten regression, where an AR(1) process is assumed. For the different definitions of conviction in Scotland and England/Wales, see the notes to Figure A.26.
** $p < 0.05$.

6. Unintended Consequences and Spillovers of the Lower Limit

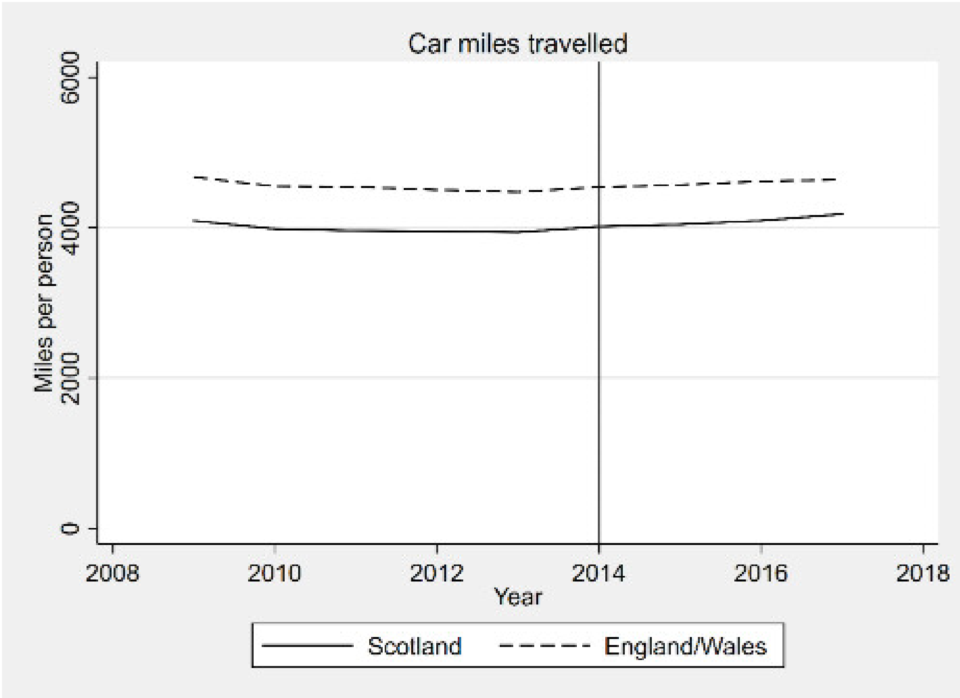
British Social Attitudes Survey (BSA) — The British Social Attitudes Survey (BSA) is a nationally representative survey of adults (aged 18 and over) living in private households in Great Britain. Participants are selected using a random probability sample that ensures the survey results are representative of the British population. The survey began in 1983 with the purpose of monitoring attitudes of a range of social issues. The survey is administered on over 3,000 people each year. New questions are added and removed over time to take into account changing attitudes and in order to reflect current events.

Figure A.27: Trends in Attitudes Toward Drink Driving: Scotland versus the Rest of Britain



Source: British Social Attitudes Surveys, 2009–2016.

Figure A.28: Trends in Annual Car Miles Travelled Per Person: Scotland versus the Rest of Britain



Source: Department for Transport Statistics (Table TRA8902).

Table A.10: Effect of the DDL Reform on Vehicle Miles Travelled — Difference-in-Difference Estimates

	Mean	(a)	(b)	(c)
β	3,992.8	30.38*	52.34	-16.55
		(18.27)	(59.06)	(22.06)
Observations		2,976	2,976	2,976
Controls		N	Y	Y
Linear annual trend		N	N	Y
Linear annual trend × Scotland		N	N	Y
LAs fixed effects		N	N	Y

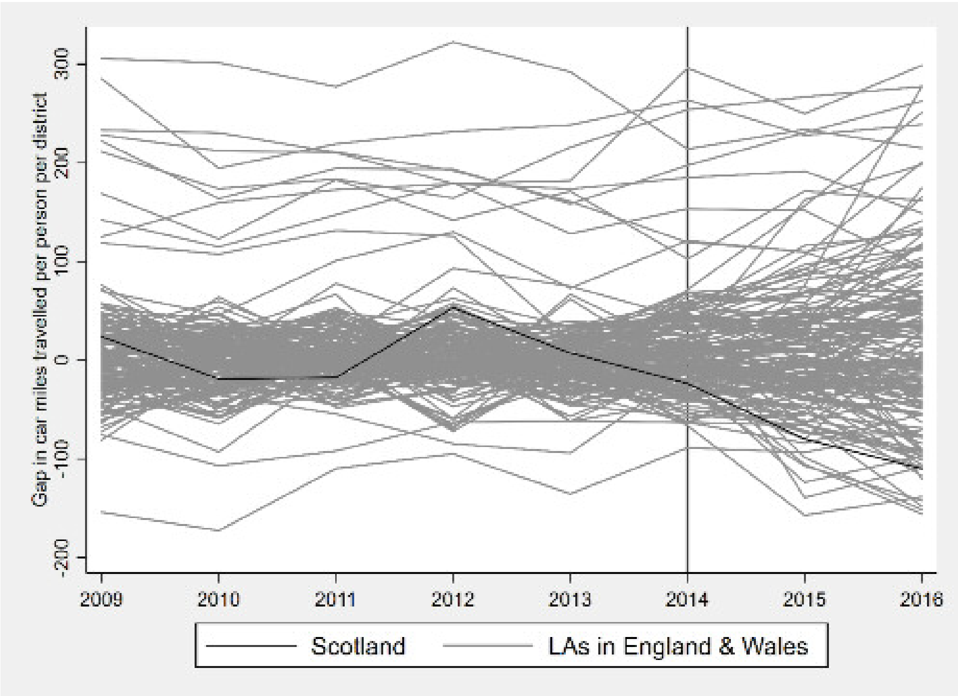
Source: Department for Transport Statistics (Table TRA8902).
Note: Observations are at the LA-year level. The sample period goes from 2009 to 2016. The dependent variable is the LA average number of car miles travelled per person per year. Standard errors are clustered at the LA level. 'Mean' refers to the Scottish pre-reform mean of the dependent variable. 'Controls' are yearly averages of temperature range, population density, proportion of residents aged 16 or more with no educational qualification, proportion of residents with bad or very bad health, median total hours worked, and median gross pay, Job Seekers' Allowance rate, alcohol licensed premises, and total road length. 'LAs' denotes local authorities.
* $p < 0.10$.

Table A.11: Effect of the DDL Reform on Vehicle Miles Travelled — Spatial Regression Discontinuity Estimates

	(a) <200 km	(b) <100 km	(c) <50 km
β	2.550	-15.82	67.28
	(29.93)	(55.61)	(59.02)
	[0.934]	[0.824]	[0.421]
Mean	4007.0	4143.0	4858.4
Observations	736	184	48
Number of LAs	92	23	6

Notes: Standard errors in parentheses are clustered at the LA level. Due to the small number of LAs, wild bootstrapped p-values computed using Webb weights (Webb, 2014) and 5,000 replications are in square brackets. For completeness, however, these are shown also for large bandwidths. For all other details, see the note to Table A.10.

Figure A.29: Gaps in Car Miles Travelled for Scotland and Synthetic Scotland and for Scotland and Placebos in Control LAs



Notes: Placebo districts with pre-reform mean squared prediction error (MSPE) that are two times higher than Scotland's are excluded.

UK Time Use Survey (UK-TUS) 2014/2015 — The UK Time Use Survey (UK-TUS) 2014/2015 conducted between April 2014 and December 2015. The sample consists of individuals aged 8 years and over living in households in England, Scotland, Wales, and Northern Ireland. 9,388 individuals in 4,238 households provided 16,553 diary days. The time diary files in the UK-TUS (2014/15) provide the following information reported over a 24 hour that runs from 4am until 4am on both a weekday and a weekend day: primary activities, secondary activities, the location where the activity took place, who the respondent was with and the level of enjoyment.

Those who completed a time diary time diary were asked to take part in an interview. In addition, a household member was chosen to complete the household interview that provides additional characteristics that we examine, such as age and gender. We have primarily use the location of the individual that indicates where someone is at a particular time. In the analysis we use all those aged 18 and over.

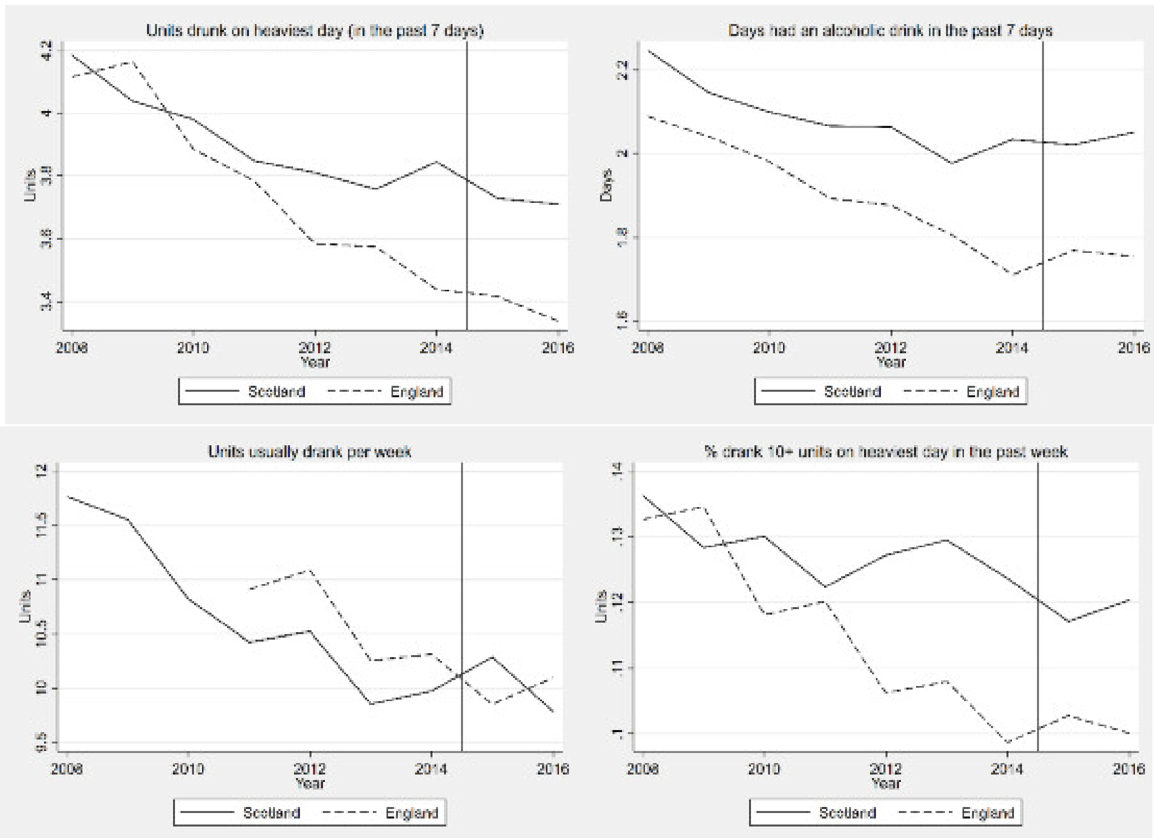
Table A.12: Effect of the DDL Reform on Time Spent Driving — Difference-in-Difference Estimates

	(a)	(b)	(c)	(d)	(e)	(f)
	A. All individuals		B. Male		C. Aged 18-30	
All Days						
β	-0.142	0.0767	-0.482	-0.260	-1.296	-1.062
	(0.440)	(0.425)	(0.660)	(0.637)	(1.183)	(1.052)
Mean	1.97		2.34		2.67	
Observations	15,527	15,527	7,112	7,112	2,842	2,842
Weekends						
β	-0.347	-0.214	-0.540	-0.406	-0.687	-0.569
	(0.309)	(0.305)	(0.472)	(0.464)	(0.693)	(0.645)
Mean	1.25		1.43		1.46	
Observations	14,816	14,816	6,761	6,761	2,674	2,674
Controls	N	Y	N	Y	N	Y

Sources: UK-TUS, 2014–2015.
Notes: Dependent variable is the number of minutes driving one's car from 6pm to midnight. "Weekends" defined as Friday, Saturday and Sunday. Fieldwork was conducted between April 2014 and December 2015. Controls include age, and indicators for: having left school at age 17 or more, gender, presence of at least one child aged 0-14 in the household, full-time employment, self-employment. Standard errors are clustered at the household level.

Health Survey of England (HSE) and the Scottish Health Survey (SHS) — The Health Survey of England (HSE) is a nationally representative survey that selects participants living in private households using a random probability sample. Every address in England has an equal chance of being included each year. The survey provides data, via interview, on a range of issues such as smoking, drinking, and dietary habits, and general health, as well as objective health measures such as height, weight and blood pressure that are collected by a visit from a specially trained nurse. The Scottish Health Survey (SHS) is very similar to the HSE and has been designed to provide data on the health of adults and children living in private households in Scotland every year. The questions in the surveys are, in many cases, the same. We have the used variables on drinking, smoking and eating where the questions in both the HSE and SHS were the same.

Figure A.30: Trends in Alcohol Consumption: Scotland versus the Rest of Britain



Sources: Health Survey of England (England) and Scottish Health Surveys (Scotland), 2008–2016. *Notes*: The outcomes are: the number of alcohol units drunk on heaviest day in the previous 7 days (top left); the number of days the interviewee drank over the past 7 days (top right); the number of alcohol units usually drunk per week, conditional on drinking, available in both England and Scotland from 2011 (bottom left); drinking 10 units of alcohol or more on the heaviest day (bottom right).

Table A.13: Effect of the DDL Reform on Time Spent in the Pub — Difference-in-Difference Estimates

	(a)	(b)	(c)	(d)	(e)	(f)
	A. All		B. Male		C. Female	
β	-2.070	-0.191	0.206	0.0139	-4.072**	-0.367*
	(1.580)	(0.159)	(1.787)	(0.179)	(2.067)	(0.206)
Mean	8.55		7.35		9.60	
Observations	8,752	8,752	4,163	4,163	4,589	4,589
	D. Aged 18-30		E. Aged 31-49		F. Aged 50+	
β	-0.972	-0.0651	-1.980	-0.146	-2.077	-0.224
	(2.209)	(0.225)	(3.108)	(0.308)	(2.429)	(0.242)
Mean	8.13		9.35		8.39	
Observations	1,790	1,790	2,711	2,711	3,901	3,901
	G. Monday - Thursday		H. Friday, Saturday, Sunday		I. Saturday, Sunday	
β	-0.728	-0.0623	-1.900	-0.154	-2.530*	-0.211
	(1.154)	(0.116)	(1.216)	(0.123)	(1.354)	(0.136)
Mean	3.71		4.64		4.72	
Observations	5,670	5,670	8,603	8,603	7,131	7,131
Controls	N	Y	N	Y	N	Y

Sources: UK-TUS, 2014–2015.
Notes: Dependent variable is the number of minutes in a pub, restaurant or cafe. Fieldwork was conducted between April 2014 and December 2015. Controls include age, and indicators for: having left school at age 17 or more, gender, presence of at least one child aged 0-14 in the household, full-time employment, self-employment. Standard errors are clustered at the household level.

Table A.14: Effect of the DDL Reform on Smoking and Eating — Difference-in-Difference Estimates

	(a)	(b)	(c)	(d)	(e)	(f)
	A. Cigarettes Per Day			B. Currently Smoking		
β	-0.302***	-0.276***	-0.013	-0.013**	-0.010*	0.003
	(0.089)	(0.086)	(0.127)	(0.006)	(0.005)	(0.008)
Mean		3.25			0.235	
Observations	129,235	129,235	129,235	129,612	129,612	129,612
	C. At Least One Portion of F&V			D. 5+ Portions of F&V		
β	-0.002	-0.003	-0.005	-0.015***	-0.014**	-0.008
	(0.004)	(0.004)	(0.006)	(0.005)	(0.005)	(0.009)
Mean		0.914			0.205	
Observations	148,981	144,248	144,248	148,981	144,248	144,248
Controls	N	Y	Y	N	Y	Y
Linear annual trend	N	N	Y	N	N	Y
Linear annual trend × Scotland	N	N	Y	N	N	Y

Sources: Health Survey of England (England) and Scottish Health Surveys (Scotland), 2008–2016.
Notes: Observations correspond to the number of individuals over the sample period. The dependent variables are: the number of cigarettes usually smoked in a day (panel A); an indicator variable that equals 1 if the individual smokes at the time of the survey, and 0 otherwise (panel B); an indicator variable that equals 1 if the individual eats at least one portion of fruit and vegetables (F&V) per day, and 0 otherwise (panel C); an indicator variable that equals 1 if the individual eats five or more portions of fruit and vegetables (F&V) per day, and 0 otherwise (panel D). 'Mean' refers to the Scottish pre-reform mean of the dependent variable. Robust standard errors are in parentheses. In all panels, 'controls' are: indicators of sex, marital status (married/cohabiting), ethnic minority (White, Black, or Asian, with others as the base category), education (leaving school at age 17 or after), and age (15 3-year age band groups).
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

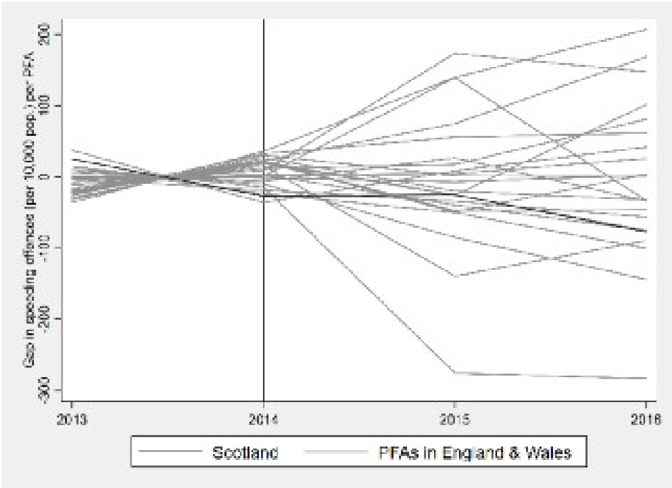
Table A.15: Effect of the DDL Reform on Speeding and Other Motor Vehicle Offenses — Difference-in-Difference Estimates

	Mean	(a)	(2)	(c)
			A. Speeding	
β	134.3	-89.00***	-73.85**	1.494
		(15.10)	(34.70)	(27.81)
Observations		164	164	164
			B. Seat Belt	
β	55.5	-30.89***	-27.64***	-23.06***
		(1.228)	(5.057)	(2.084)
Observations		246	246	246
			C. Mobile Phone	
β	50.4	-30.13***	-31.46***	-25.42***
		(1.123)	(3.481)	(1.950)
Observations		246	246	246
Linear annual trend		N	N	Y
Linear annual trend $\times$ Scotland		N	N	Y
PFAs fixed effects		N	Y	Y

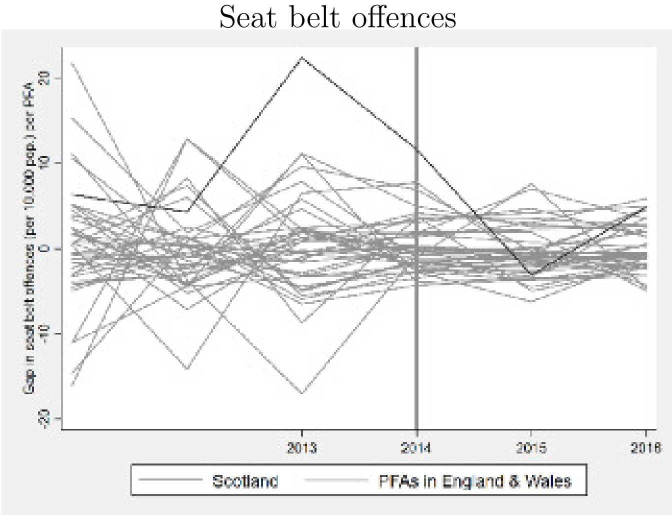
Sources: Recorded Crime in Scotland (Scotland); Fixed Penalty Notices for Motoring Offences Statistics Data Tables: Police Powers and Procedures, Home office (England and Wales).
Notes: Observations are at the PFA-year level. The dependent variable is the number of crimes/offences per 10,000 heads of population. For panel A, the sample period goes from 2013 to 2016. For panel B and C, the sample period goes from 2011 to 2016. Standard errors in parentheses are clustered at the PFA level. 'Mean' refers to the Scottish pre-reform mean of the dependent variable. All regressions include, as controls, yearly averages of temperature range, population density, proportion of residents aged 16 or more with no educational qualification, proportion of residents with bad or very bad health, median total hours worked, and median gross pay, Job Seekers' Allowance rate, alcohol licensed premises, and total road length. 'PFAs' denotes police force areas.
** $p < 0.05$, *** $p < 0.01$.

Figure A.31: Gaps in Speeding and Other Motor Vehicle Offenses for Scotland and Synthetic Scotland and for Scotland and Placebos in Control PFAs

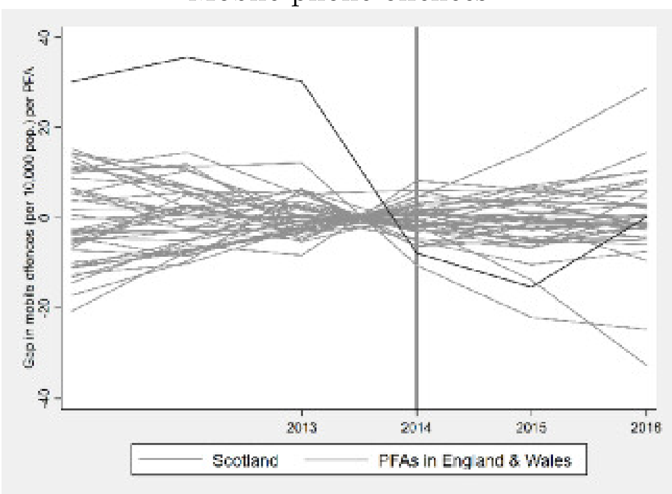
Speeding



Seat belt offences



Mobile phone offences



Notes: In all panels, placebo police force areas with pre-reform mean squared prediction error (MSPE) that are two times higher than Scotland's are excluded.

Other Types of Crime — Here we define each of the four crime types analyzed in the text, namely drug, attempted murder and serious assault, robbery, and sexual crime for Scotland and England and Wales separately.

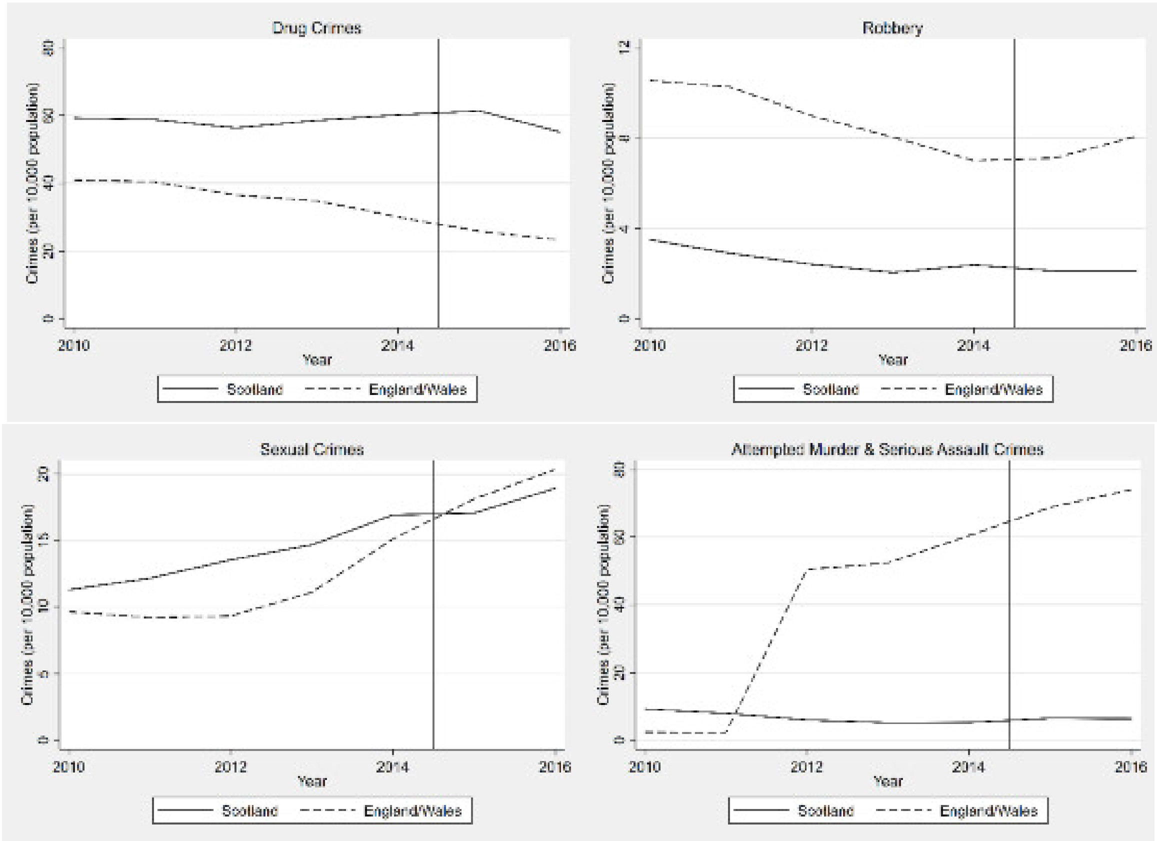
Drug crime — In Scotland, drug crime is defined by the following activities: illegal importation of drugs, illegal cultivation of drugs, possession of drugs with intent to supply, possession of drugs, and "Drugs, other offenses, money laundering". For England and Wales, instead, it is defined by: other drug offenses, possession of controlled drugs (cannabis), possession of controlled drugs (excluding cannabis), and trafficking in controlled drugs.

Attempted murder and serious assault — In Scotland, the crime comprising attempted murder and serious assault is defined by the following activities: attempted murder and serious assault. For England and Wales, instead, it is defined by: attempted murder, assault with injury, assault without injury, inflicting grievous bodily harm without intent.

Robbery — In Scotland, robbery is defined by one of the following activities: robbery or robbery and assault with intent to rob. For England and Wales, instead, robbery is defined as: robbery of personal property and robbery of business property.

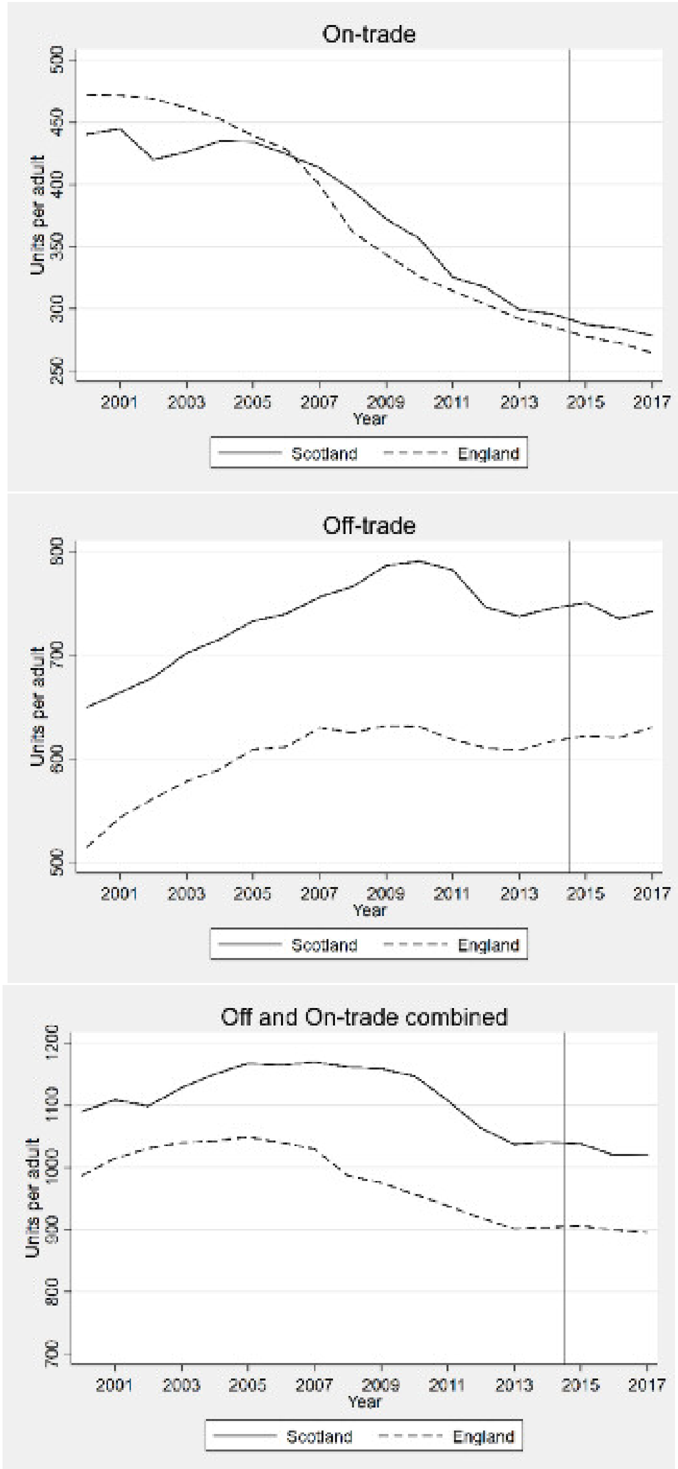
Sexual crime — In Scotland, this is defined by the following offenses: rape, attempted rape, contact sexual assault (13–15 year old or adult 16+), sexually coercive conduct (on 13–15 year old child or adult aged 16+), sexual crimes against children under 13 years of age, lewd and libidinous practices, crimes relating to prostitution, soliciting services of person engaged in prostitution, brothel keeping, immoral traffic, procuration, other sexually coercive conduct, other sexual crimes involving 13-15 year old children, taking, distribution, possession etc. of indecent photos of children, incest, unnatural crimes, public indecency, sexual exposure, threatening to disclose and intimate image, disclosure of an intimate image communications Act 2003 (sexual), other sexual crimes. For England and Wales, instead, sexual crime is defined as: abuse of children through prostitution and pornography, abuse of children through prostitution and pornography, abuse of children through prostitution and pornography, abuse of children through prostitution and pornography, exploitation of prostitution, exploitation of prostitution, incest or familial sexual offences, other miscellaneous sexual offences, rape of a female aged 16 and over, rape of a female child under 13, rape of a female child under 16, rape of a male aged 16 and over, rape of a male child under 13, rape of a male child under 16, sexual activity etc with a person with a mental disorder, sexual activity involving a child under 13, sexual activity involving a child under 13, sexual assault on a female aged 13 and over, sexual assault on a female child under 13, sexual assault on a male aged 13 and over, sexual assault on a male child under 13 years of age, sexual grooming, soliciting for the purposes of prostitution, trafficking for sexual exploitation, unnatural sexual offences.

Figure A.32: Trends in Other Crimes and Offences: Scotland versus the Rest of Britain



Sources: Recorded Crime in Scotland, 2010–2016 (Scotland); Recorded crime data at the community safety partnership and local authority level (ONS), 2010–2016 (England).
Notes: Each outcome is the number of crimes/offences per 10,000 heads of population. The sample period goes from 2010 to 2016. The panels are: drug crimes (top left), robbery (top right), sexual offenses (bottom left) and attempted murder & serious assault (bottom right).

Figure A.33: Trends in Aggregate Alcohol Sales: Scotland versus the Rest of Britain



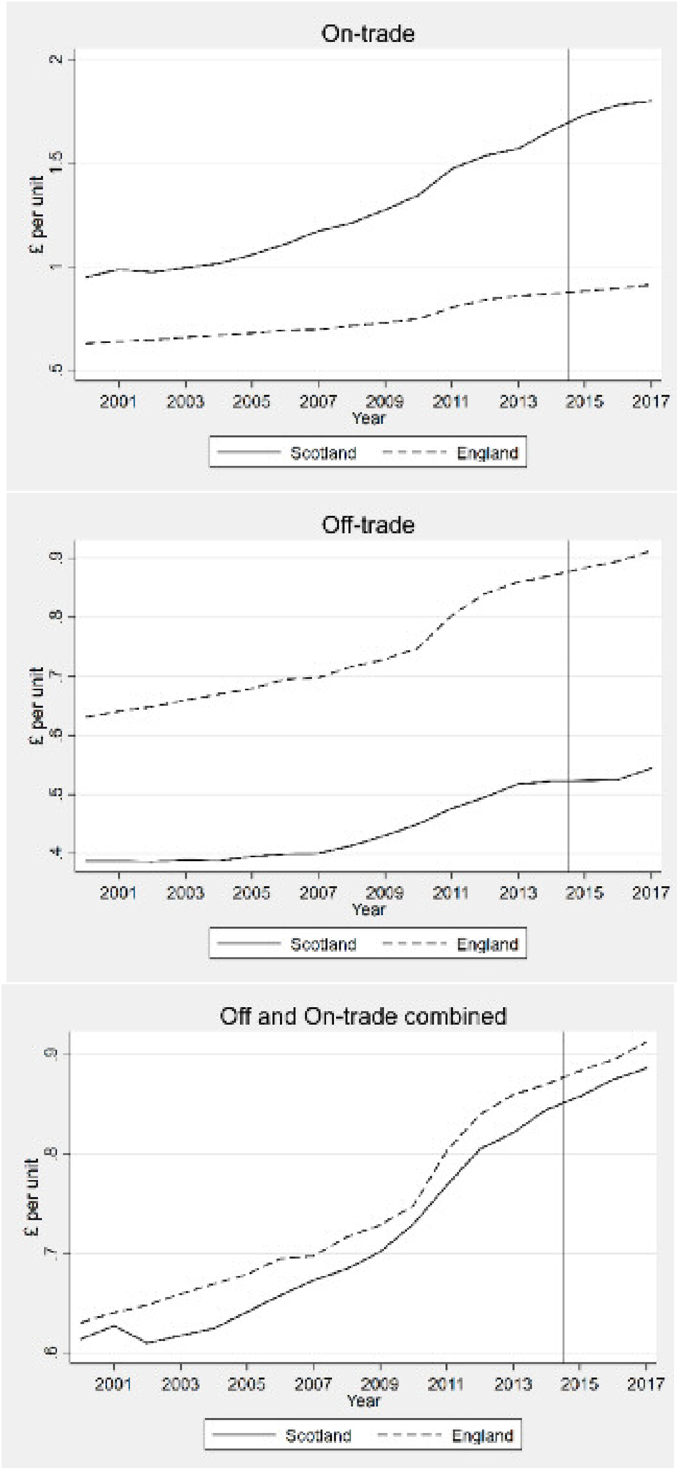
Source: Nielsen/CGA 2018: <`www.tinyurl.com/2018MESASSAS`>.
Note: The figures refer to units of pure alcohol sold per adult (aged 16 or more).

Table A.16: Effect of the DDL Reform on Alcohol Sales — Difference-in-Difference Estimates

	(a)	(b)	(c)	(d)	(e)	(f)
	Total volume of pure alcohol (units) sold per adult (aged 16+ years)					
	On-trade		Off-trade		On and off trade	
β	1.807	-11.10	-8.403	-5.926	-14.13	-14.05
	(12.03)	(10.89)	(10.07)	(10.76)	(18.70)	(15.98)
Mean	387		733		1119	
Observations	35	35	35	35	35	35
Linear annual trend	N	Y	N	Y	N	Y
Linear annual trend × Scotland	N	Y	N	Y	N	Y

Source: Nielsen/CGA 2018, available at:
<www.tinyurl.com/2018MESASSAS>.
Notes: Observations are at the country-year level. The sample period goes from 2000 to 2016. England and Wales combined are the control country. Panel-corrected standard errors are calculated using a Prais-Winsten regression, where a region-specific AR(1) process is assumed. This also allows the error terms to be region specific, heteroskedastic, and contemporaneously correlated across regions.

Figure A.34: Trends in Aggregate Alcohol Prices: Scotland versus the Rest of Britain



Source: Monitoring and Evaluating Scotland's Alcohol Strategy, Monitoring Report 2018; available at: <`www.tinyurl.com/2018MESASSAS`>

Note: The figures refer the average price per unit of alcohol.

Table A.17: Effect of the DDL Reform on Alcohol Prices — Difference-in-Difference Estimates

	(a)	(b)	(c)	(d)	(e)	(f)
	Average price per unit of alcohol sold					
	On-trade		Off-trade		On and off trade	
β	-0.015	0.019	-0.007*	-0.002	-0.008	0.003
	(0.025)	(0.022)	(0.004)	(0.003)	(0.010)	(0.009)
Observations	35		35		35	
Mean	0.43		0.489		0.695	
Linear annual trend	N	Y	N	Y	N	Y
Linear annual trend × Scotland	N	Y	N	Y	N	Y

Source: Monitoring and Evaluating Scotland's Alcohol Strategy, Monitoring Report 2018; available at: <www.tinyurl.com/2018MESASSAS>

Notes: Observations are at the country-year level. The sample period goes from 2000 to 2016. England and Wales combined are the control country. Panel-corrected standard errors are calculated using a Prais-Winsten regression, where a region-specific AR(1) process is assumed. This also allows the error terms to be region specific, heteroskedastic, and contemporaneously correlated across regions.

* $p < 0.10$.

Table A.18: Effect of the DDL Reform on the Pub Industry — Difference-in-Difference Estimates

	Mean	(a)	(b)	(c)	(d)
		A. Pubs per 1,000 Population			
β	0.567	0.0273**	0.0245**	0.00537	-0.0103
		(0.0133)	(0.0111)	(0.0367)	(0.0243)
Observations		3,008	3,008	3,008	3,008
		B. Pub Jobs per 1,000 Population			
β	4.80	-0.412***	-0.493**	0.172	-0.194
		(0.122)	(0.237)	(0.555)	(0.329)
Observations		3,008	3,008	3,008	3,008
Controls		N	Y	Y	Y
Linear annual trend		N	N	Y	Y
Linear annual trend × Scotland		N	N	Y	Y
LAs fixed effects		N	N	N	Y

Source: "Economies of Ale: Changes in the UK Pubs and Bars Sector, 2001 to 2019", ONS; available at: <https://www.tinyurl.com/econale2020>

Notes: Observations are at the LA-year level. The sample period goes from 2009 to 2016. The dependent variables are the number of pubs per 1,000 heads of population (panel A) and the number of pub jobs per 1,000 heads of population (panel B). Standard errors in parentheses are clustered at the LA level. 'Mean' refers to the Scottish pre-reform mean of the dependent variable. 'Controls' are LA annual averages of temperature range, population density, proportion of residents aged 16 or more with no educational qualification, proportion of residents with bad or very bad health, median total hours worked, median gross pay, Job Seekers' Allowance rate, alcohol licensed premises, and total road length. 'LAs' denotes local authorities.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.19: Effect of the DDL Reform on the Pub Industry — Spatial Regression Discontinuity Estimates

	(a) <200 km	(b) <100 km	(c) <50 km
	A. Pubs per 1,000 Population		
β	-0.043***	-0.066**	-0.139
	(0.014)	(0.026)	(0.088)
	[0.004]	[0.017]	[0.164]
Mean	0.579	0.612	0.618
	B. Pub Jobs per 1,000 population		
β	-0.700***	-0.951**	-2.194
	(0.156)	(0.344)	(1.130)
	[0.000]	[0.006]	[0.084]
Mean	4.821	4.865	4.222
Observations	752	200	56
Number of local authorities	94	25	7
Linear annual trend	N	Y	Y
Linear annual trend × Scotland	N	Y	Y
LA fixed effects	N	N	Y

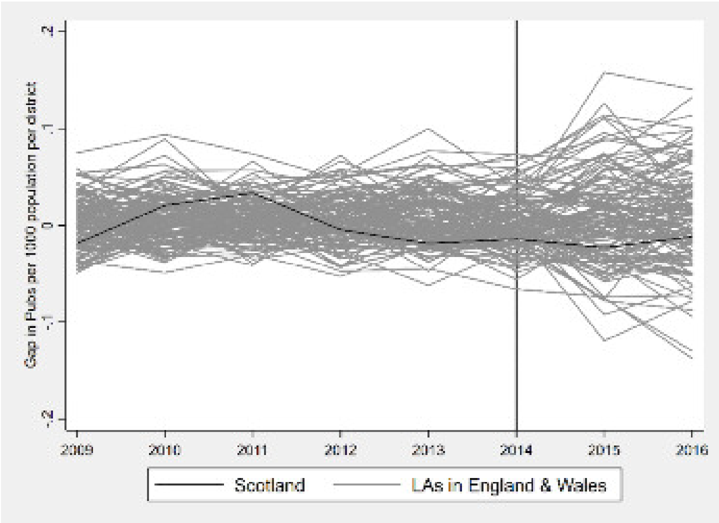
Source: "Economies of Ale: Changes in the UK Pubs and Bars Sector, 2001 to 2019", ONS; available at: <www.tinyurl.com/econale2020>

Note: Observations are at the LA-year level. The sample period goes from 2009 to 2016. The dependent variables are the number of pubs per 1,000 heads of population (panel A) and the number of pub jobs per 1,000 heads of population (panel B). Standard errors in parentheses are clustered at the LA level. 'Mean' refers to the Scottish pre-reform mean of the dependent variable. All regressions are estimated with all controls as in Table A.18. Due to the small number of LAs, wild bootstrapped p-values computed using Webb weights (Webb, 2014) and 5,000 replications are in square brackets. For completeness, however, these are shown also for large bandwidths. All regressions control for distance from the Scottish/English border and distance from the border interacted with Scotland (with English distances taking negative values) are also included. 'LAs' denotes local authorities.

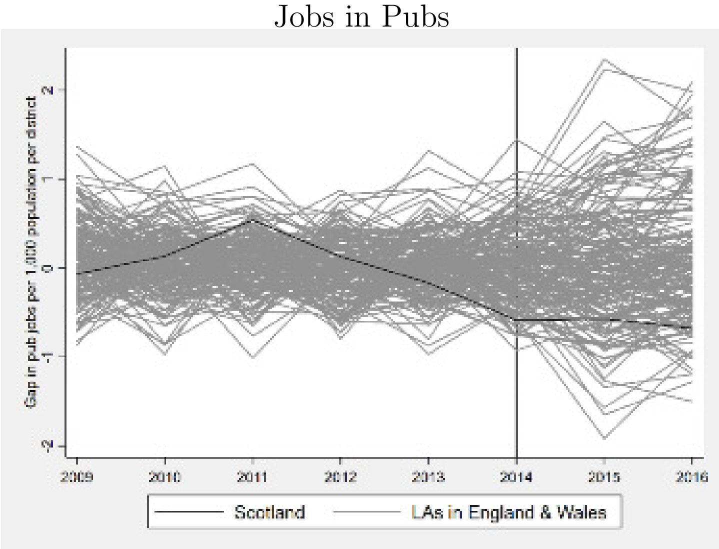
** $p < 0.05$, *** $p < 0.01$.

Figure A.35: Gaps in Pubs and Pub Jobs per 1,000 Population for Scotland and Synthetic Scotland and for Scotland and Placebos in Control LAs

Pubs



Jobs in Pubs



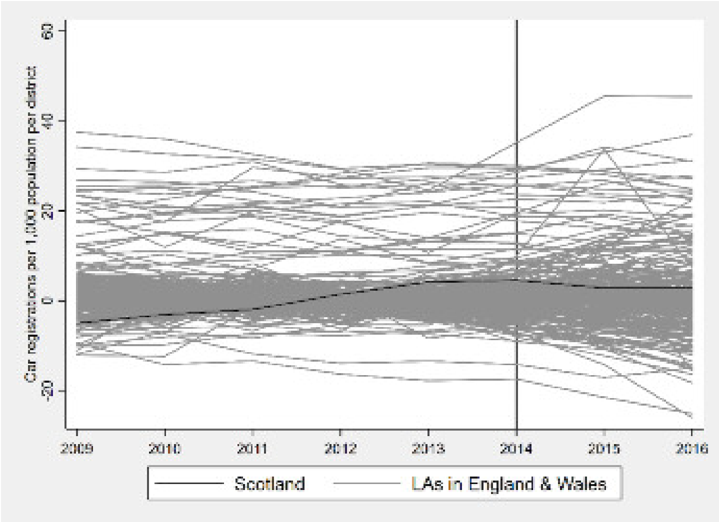
Notes: In all panels, placebo LAs with pre-reform mean squared prediction error (MSPE) that are two times higher than Scotland's are excluded.

Table A.20: Effect of the DDL Reform on Car Registrations — Difference-in-Difference Estimates

	Mean	(a)	(b)	(c)
β	450.7	4.391	6.995	-0.075
		(4.033)	(5.282)	(3.772)
Observations		3,024	3,024	3,024
Controls		N	Y	Y
Linear annual trend		N	N	Y
Linear annual trend $\times$ Scotland		N	N	Y
LA fixed effects		N	N	Y

Notes: Observations are at the LA-year level. The sample period goes from November 2009 to December 2016. The dependent variable is the number of registered cars at the LA level. Standard errors in parentheses are clustered at the LA level. 'Mean' refers to the Scottish pre-reform mean of the dependent variable. 'Controls' are LA annual averages of temperature range, population density, proportion of residents aged 16 or more with no educational qualification, proportion of residents with bad or very bad health, median total hours worked, median gross pay, Job Seekers' Allowance rate, alcohol licensed premises, and total road length. 'LA' denotes local authority.

Figure A.36: Gaps in Car Registration Rates for Scotland and Synthetic Scotland and for Scotland and Placebos in Control LAs



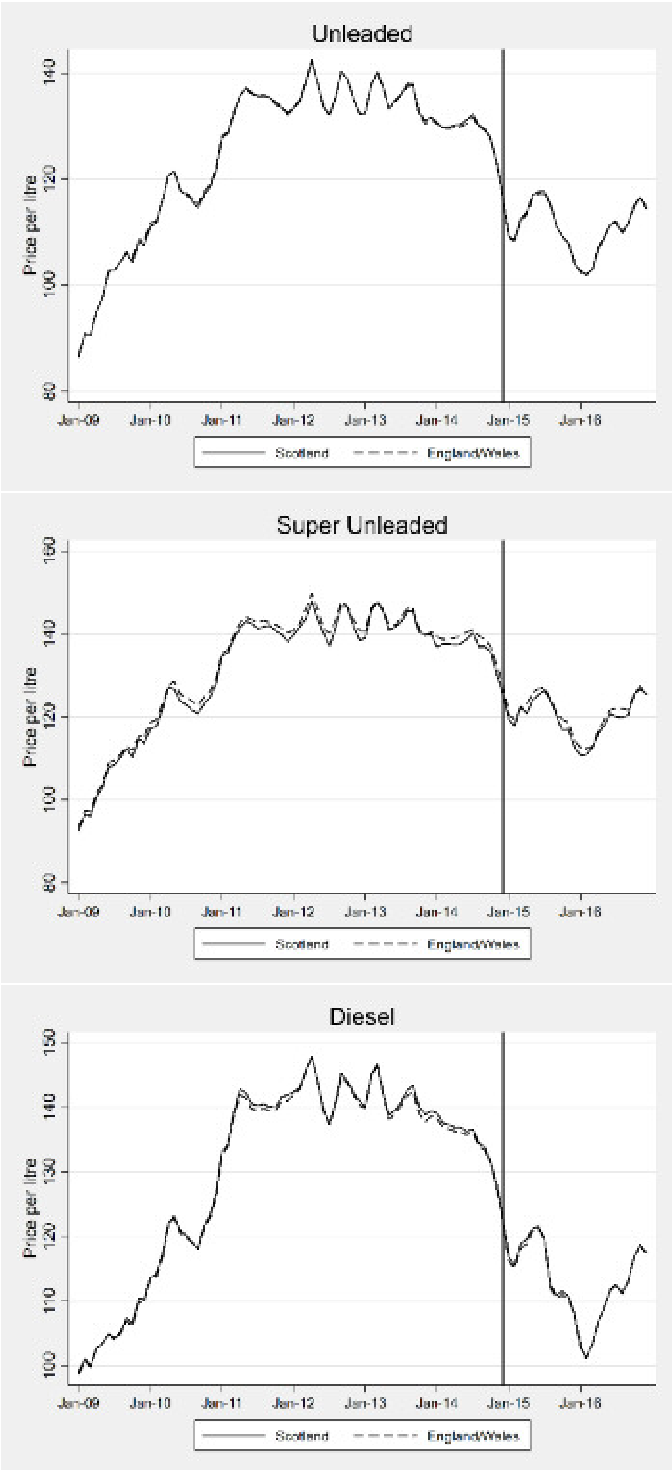
Notes: In all panels, placebo LAs with pre-reform mean squared prediction error (MSPE) that are two times higher than Scotland's are excluded. 'LAs' denotes local authorities.

Table A.21: Effect of the DDL Reform on Car Registrations — Spatial Regression Discontinuity Estimates

	(a) <200 km	(b) <100 km	(c) <50 km
β	-2.056	-3.156**	-3.965
	(3.535)	(1.510)	(2.526)
	[0.624]	[0.042]	[0.137]
Mean	447.3	441.2	482.5
Observations	768	208	56
Number of local authorities	96	26	7
Linear annual trend	N	Y	Y
Linear annual trend $\times$ Scotland	N	Y	Y
LAs fixed effects	N	N	Y

Note: Observations are at the LA-year level. The sample period goes from 2009 to 2016. The dependent variables are the number of pubs per 1,000 heads of population (panel A) and the number of pub jobs per 1,000 heads of population (panel B). Standard errors in parentheses are clustered at the LA level. 'Mean' refers to the Scottish pre-reform mean of the dependent variable. All regressions are estimated with all controls as in Table A.20. Due to the small number of LAs, wild bootstrapped p-values computed using Webb weights (Webb, 2014) and 5,000 replications are in square brackets. For completeness, however, these are shown also for large bandwidths. All regressions control for distance from the Scottish/English border and distance from the border interacted with Scotland (with English distances taking negative values) are also included. 'LAs' denotes local authorities.
** $p < 0.05$.

Figure A.37: Trends in Average Petrol Prices: Scotland versus the Rest of Britain



Source: Automobile Association (The AA) Fuel Price Reports, January 2009–December 2016; available at: <www.tinyurl.com/AAfuelprices>

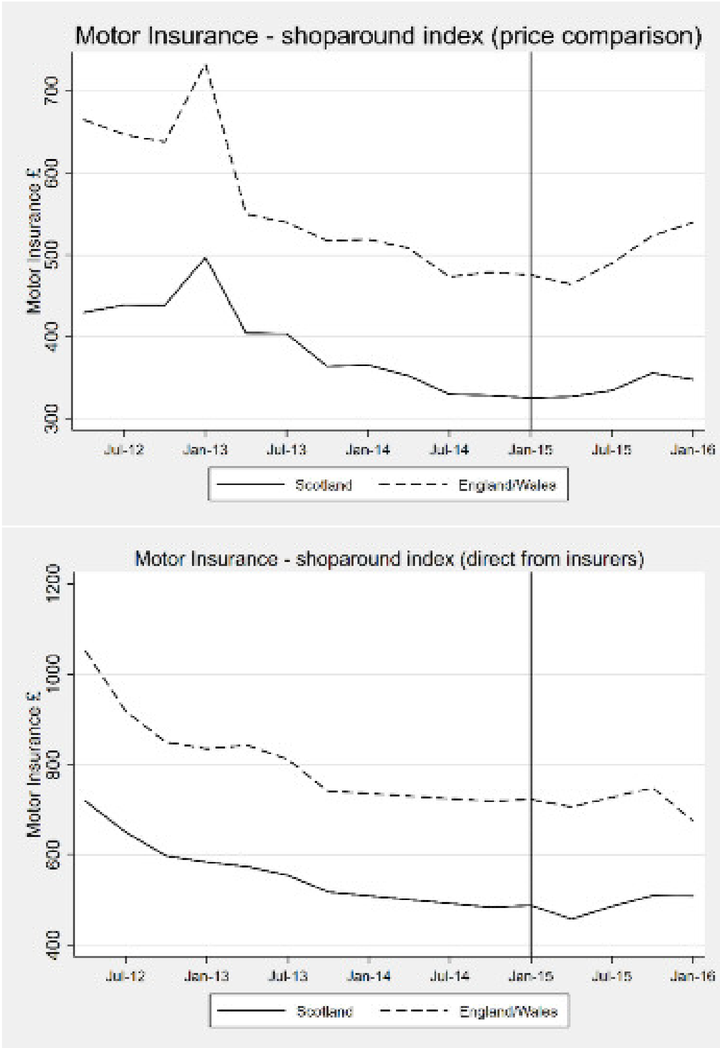
Table A.22: Effect of the DDL Reform on Petrol Prices — Difference-in-Difference Estimates

	(a)	(b)	(c)	(d)	(e)	(f)	(g)	(h)	(i)
	Pence per litre								
	A. Unleaded			B. Super Unleaded			C. Diesel		
β	-0.299	-0.586**	-0.461*	-0.116	-0.302	0.033	0.020	-0.108	-0.058
	(0.246)	(0.245)	(0.243)	(0.813)	(0.793)	(0.806)	(0.205)	(0.202)	(0.210)
Mean		125.2			131.7			130.6	
Observations	1,045	1,045	1,045	1,045	1,045	1,045	1,045	1,045	1,045
Month-year trend	N	Y	Y	N	Y	Y	N	Y	Y
Month-year trend × Scotland	N	Y	Y	N	Y	Y	N	Y	Y
Month FEs	N	N	Y	N	N	Y	N	N	Y
Month FEs × Scotland	N	N	Y	N	N	Y	N	N	Y

Source: Automobile Association (The AA) Fuel Price Reports, January 2009–December 2016; available at: <www.tinyurl.com/AAfuelprices>
Notes: Observations are at the region-month-year level. The sample period goes from January 2009 to December 2016, except London which runs from January 2009 to January 2016. Control regions are: London, South West, South East, East Anglia, East Midlands, West Midlands, Yorkshire and Humberside, North West, North Wales. The dependent variable is the price (pence) per litre of petrol. 'Mean' refers to the Scottish pre-reform mean of the dependent variable. 'FEs' denotes fixed effects. Panel-corrected standard errors are calculated using a Prais-Winsten regression, where a region-specific AR(1) process is assumed. This also allows the error terms to be region specific, heteroskedastic, and contemporaneously correlated across regions.
* $p < 0.10$, ** $p < 0.05$.

Automobile Insurance Premiums — The Automobile Association (AA) publishes a variety of motor insurance indexes, which are widely used in the automobile industry. The most consistent time series is on the price comparison "shoparound" premium index (also known as aggregator). The data go from April 2012 to January 2016 and are collected quarterly. The shoparound premium is a combined average of the five cheapest quotes from both the price comparison site market and from the direct and broker market. There is also a shoparound direct series (i.e., directly from insurers). The data for this index cover the same time period as the price comparison index, except there is missing information for the three quarters from January 2014 to September 2014. The data are regional (TV regions), with the following breakdown: Anglia, Border Tyne Tees, Central, Granada, London, South, Wales, West and Yorkshire, and Scotland.

Figure A.38: Trends in Car Insurance Premiums: Scotland versus the Rest of Britain



Source: The Automobile Association.

Table A.23: Effect of the DDL Reform on Shoparound Automobile Insurance Premium Indices — Difference-in-Difference Estimates

	Mean	(a)	(b)	(c)
		A. Premium		
β	395.4	-9.039	8.079	-28.84
		(29.56)	(40.84)	(25.27)
Observations		160	160	160
		B. Direct Premium		
β	584.5	-14.54	-3.369	-40.69
		(32.55)	(33.34)	(26.34)
Observations		160	160	160
Linear quarterly trend		N	Y	Y
Linear quarterly trend $\times$ Scotland		N	Y	Y
TV region fixed effects		N	N	Y

Notes: Observations are at the TV region-quarter-year level. The sample period goes from April 2012 to January 2016 for panel A, and the same period except the three quarters from January 2014 to September 2014 for which there is missing information for panel B. The dependent variable is the region specific average price of the car insurance premium (see the description above for an explanation of the two indexes). 'Mean' refers to the Scottish pre-reform mean of the dependent variable. The regions are: Anglia, Border Tyne Tees, Central, Granada, London, South, Wales, West and Yorkshire, and Scotland. Panel-corrected standard errors are calculated using a Prais-Winsten regression, where a region-specific AR(1) process is assumed. This also allows the error terms to be region specific, heteroskedastic, and contemporaneously correlated across regions.

Additional Reference

European Transport Safety Council. 2016. Case Study – Scotland's new drink driving laws. Available at <https://etsc.eu/wp-content/uploads/Case-Study-ScotlandFinal.pdf>.